3

70 years of experience

- accelerators (gas, hydride targets)
- energy sources in stars.
- thermonuclear weapons ≥ 1952 year.
- muon-catalyzed fusion.

1947 F. C. Frank (*Nature*) theory

1957 L. W. Alvarez (discovery)

d, d, μ^- $m_\mu = 207\ m_e$

fusion rate per pair

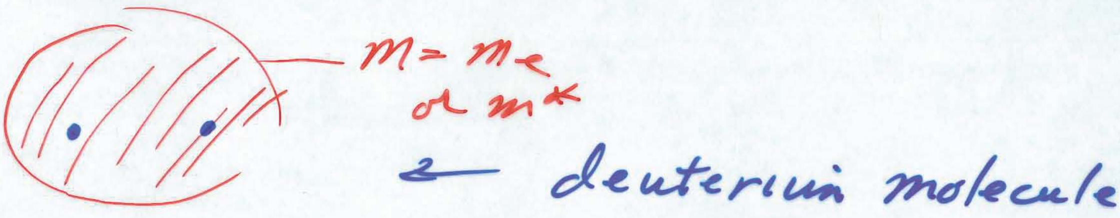
$$\Lambda = A\,|\psi(0)|^2$$

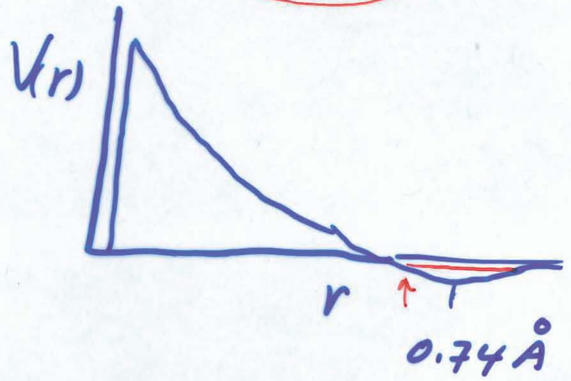
$$\sigma(E) = \frac{S(E)}{E}\, e^{-2\pi\eta}; \quad \eta = \frac{e^2}{(2E\hbar^2/\underset{\uparrow}{\mu})^{1/2}}$$

$$A = \frac{S(E=0)}{\mu c^2}\,\frac{c}{\pi\alpha}$$

$$e^{-2\pi\eta} \simeq B = e^{-4.13\sqrt{\mu}}$$

[Koonin, Nauenberg]
1989



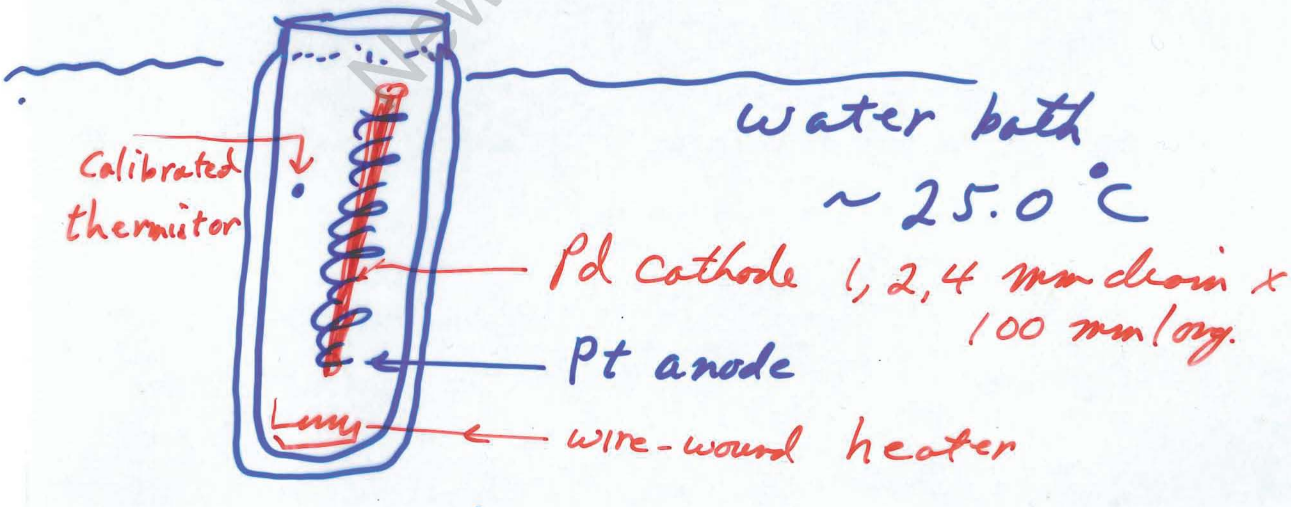


reaction rate about that in solid with $N_d \sim \left(\frac{1}{0.74\,Å}\right)^3 = 2.5\times10^{24}$

or 8 g/cm³.

03/23/89 Brigham Young U. [Utah], S.E. Jones, et al.
~ 1 n/s electrolysis of D_2O on Ti.

03/23/89 U. of Utah (Fleischman, Pons, Hawkins)
$10^3 - 10^4$ n/s; 1 – 10 W/cm³ heat.
electrolysis of LiOD in D_2O on Pd.



e.g. 200 mA; 2 – 5 V; 0.08 cm³ Pd
"0.5 W cm^{-3}" (of Pd)

? heat: $10^{-10}\ s^{-1}$; ? neutrons $10^{-20} - 10^{-23}\ s^{-1}$

05/24/89

[2]

BYU ~ 1 n/s ; ~ 1 f/s

UU – Heat $10\ \mathrm{W/cm^3}$?

– tritium 100 dpm/ml X

– He^4 "1–10 ppm" x

– neutrons (γ) 4,000/s X

Does $\dot{Q} > V \cdot I$? **

Examples: (~10 ml of electrolyte)

Tritium

100 cpm(T)/ml $\varepsilon = 30\%$ $\lambda = \frac{1}{20\,\mathrm{yrs}} = 1.6\times10^{-9}\,\mathrm{s}^{-1}$

$= 10^{-7}\,\mathrm{min}^{-1}$

$\rightarrow \times \frac{1}{0.30} \times \frac{1}{10^{-7}} \times 10 \rightarrow 3\times10^{10}$ atoms.

If 50:50 fusion ratio, and T is created

in	one day	one week	one month
n/sec	3×10^5	5×10^4	10^4
at 10^6 cpm/ml, n/sec:	3×10^9	5×10^8	10^8

If T from fusion, where are the neutrons?

05/24/89
[3]

He^4

Assume 1 watt of electrolysis (1.53 V× 0.6A)
results in 1 W of fusion $D+D \xrightarrow{(23.8\,MeV)} He^4$,

and that all He^4 diffuses out.

In off-gas, $[He^4] \times 23.8 \times 10^6 = [D_2] \times 3.06$

so $[He^4]/[D_2] = \underline{0.12\ ppm}$

vs. "1-10 ppm" observed.

But negligible He^4 can emerge < 100 C.
so $[He^4]/[D_2] << 0.1$ ppm.

05/24/89

[4]

"Neutron" bursts?

1) neutrons must diffuse out of moderator with $N = N_0 e^{-t/t_0}$ for long ~~ts~~ t.

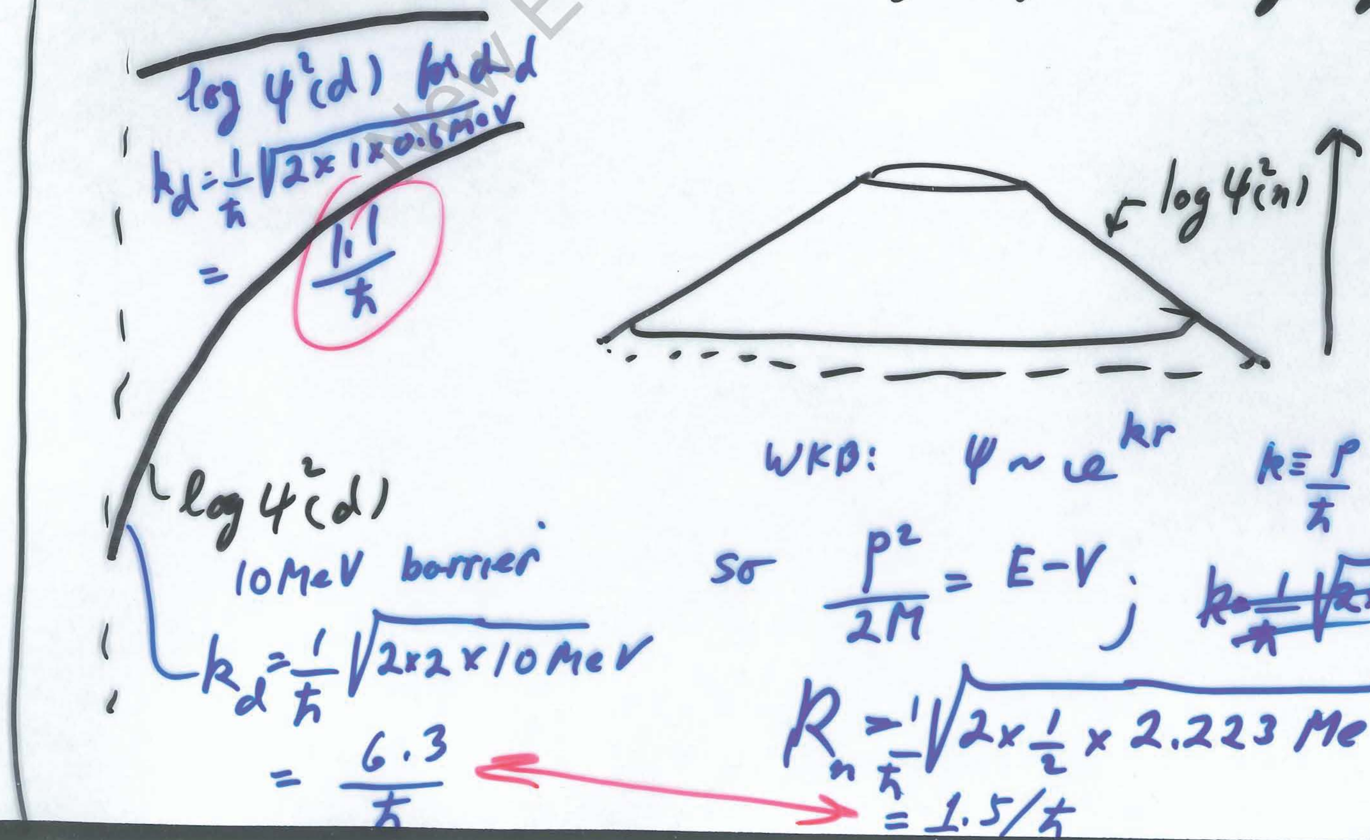
2) Frascati #2 shows no sign of bursts.

**

$d + d \rightarrow p + t$ & not $n + He^3$?

Oppenheimer – Philips reaction.

"d acts like n sometimes, despite charge symmetry"



$k_d = \frac{1}{\hbar}\sqrt{2 \times 1 \times 0.6\,MeV} = \boxed{\frac{1.1}{\hbar}}$

10 MeV barrier

$k_d = \frac{1}{\hbar}\sqrt{2 \times 2 \times 10\,MeV} = \frac{6.3}{\hbar}$

WKB: $\psi \sim e^{kr}$ $k \equiv \frac{p}{\hbar}$

so $\frac{p^2}{2M} = E - V$; $\cancel{k = \frac{1}{\hbar}\sqrt{2 \times \frac{1}{2}}}$

$k_n = \frac{1}{\hbar}\sqrt{2 \times \frac{1}{2} \times 2.223\,MeV} = 1.5/\hbar$

05/24/89

How obtain a few neutrons?

Remember pyroelectrics:

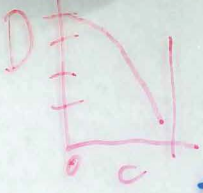
Fields $\sim (300) \times 2\pi \times (4.8 \times 10^{-10}) \times 10^{15}\,\text{cm}^{-2}$

$\sim 10^{9}$ (or 10^{8}) V/cm

Current density

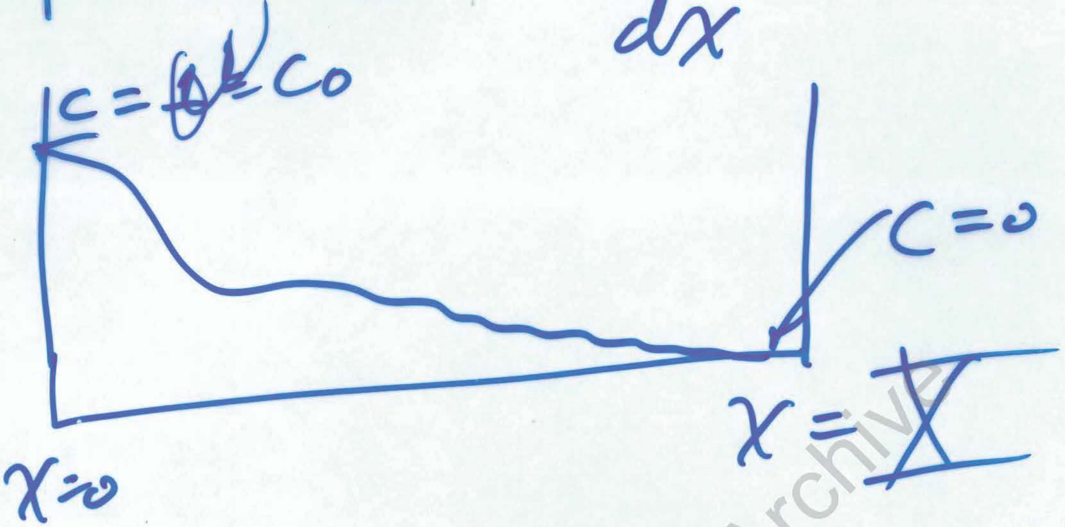
$$J \sim \frac{4.8 \times 10^{-10} \times 10^{15}}{3 \times 10^{9}} \frac{1}{\tau} \ \text{amp/cm}^2$$

For $\tau = 10^{-7}$ sec, $J \sim \underline{2000}\ \text{amp/cm}^2$.



But does not significantly impede diffusion.

$$F = D(c)\frac{dc}{dx} = \text{const.}$$



$$\text{let } \psi = \int_0^{c} D(c')\,dc' \equiv \int_0^{c} \langle D(c')\rangle\, dc'$$

$$\frac{d\psi}{dx} = D(c)\frac{dc}{dx} = F$$

So if we maintain a surface $C = C_0$

$$F = \langle D(c)\rangle_0^{c_0}\frac{c_0}{X}$$

↑ average over $\underline{c}$ from 0 to c_0

```
Received: by CERNVM (Mailer R2.02) id 4404; Tue, 23 May 89 17:45:34 GVA
Date:          Tue, 23 May 89 17:34:19 GVA
From:          GORDON <SHAWG@CERNVM>
Subject:       COLD FUSION
To:            RLG2@YKTVMV
```

Thanks for your Bitnet message. I don't think that the bubble chamber fusion would be seen since there are probably not many free Qs in liquid D and if there was one the short tracks from Q plus He(4) would probably not be noticed. The second point about further suppresion of the neutrons could be from the 3 body phase-space and the energy range where the n's were looked for. However, I absolutely agree with you that once a Q is on Pd it would be Extremely hard to get off. That is certainly a weak point and we have revised our preprint to include this. Please let me know what happens at the workshop. I will be here to May 31. Gordon Shaw

WHAT'S NEW, Friday, 19 May 1989 Washington, DC

1. LAUNCH OF THE HUBBLE SPACE TELESCOPE HAS BEEN DELAYED AGAIN! The planned Dec 89 launch of Hubble (WN 2 Sep 88) will be put off at least until Apr 90 to make way for secret military missions of the shuttle. Hubble is already 3 years late, and the delay costs $6 per second. Scientists had to choose between Hubble and those missions with specific launch windows. Once in orbit, Hubble may have trouble calling home; the Pentagon is commandeering more of the Tracking and Data Relay Satellite System than expected.

2. THE GAMMA RAY OBSERVATORY WILL ALSO BE HELD UP, but it is the Soviet military that will create the major headaches for the GRO. The nuclear reactors that power Soviet radar satellites (WN 20 Jan 89) are 50 times brighter gamma sources than the crab nebula. The GRO will be blinded every time the Soviet reactors pass by.

3. THE COLD FUSION PANEL OF THE ENERGY RESEARCH ADVISORY BOARD will be co-chaired by physicist Norman Ramsey and nuclear chemist John Huizenga. The distinguished 20-member committee was formed at the request of Energy Secretary Watkins (WN 28 Apr 89), who asked for a preliminary report on experimental and theoretical results by Aug 31. The final coroners report is due Nov 15.

4. A PALLADIUM CURTAIN HAS DESCENDED OVER THE UNIVERSITY OF UTAH. In spite of the $400 ticket price, a near-capacity crowd of 1500 is expected at the May 23-25 Workshop on Cold Fusion Phenomena in Santa Fe. Although the preliminary program gives Fleischmann and Pons top billing, the temperamental stars apparently decided to cancel their appearance. Plans to invite K.G. Lynn of BNL to give a colloquium at Utah were also cancelled when Pons objected. Lynn is coauthor of an invited paper at Santa Fe that reports no evidence of cold fusion. University lawyers are said to have been worried that Pons might feel pressured to reveal the secret formula. The Santa Fe non-confrontation will be broadcast live via satellite. For details on pickup call David Phillips, LANL, (505) 667-1233. The APS cold fusion sessions from the Baltimore meeting are now available on video tape. The tape costs only one-fourth as much as registration at Santa Fe, and also doesn't have Fleischmann and Pons. Call APS Cold Fusion (212) 682-7341.

5. THE OTHER KIND OF FUSION WAS BACK IN THE NEWS THIS WEEK after the leak of a National Academy of Sciences report on magnetic confinement fusion. The report calls for accelerated construction of the $455M Compact Ignition Tokamak to produce a sustained reaction in the early 90's. It would be followed by construction of a prototype fusion power plant, the International Tokamak Experimental Reactor, around the end of the century. The report was completed in March, but the Academy had no desire to invite the inevitable comparisons of tokamaks and Rubbermaid dishpans during the peak of the cold fusion hysteria. In terms of energy gain, tokamaks and palladium cathodes are currently about even.

Robert L. Park (202) 232-0189 The American Physical Society

OPTIONS: ACK LOG LONG NOTEBOOK *
Local options: Search RealNode

Date: 18 May 1989, 15:34:55 EDT
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
P.O. Box 218
Yorktown Hts, NY 10598
To: ZIEGLER at YKTVMV
Subject: Poster presentation in Santa Fe.
Reply-To: RLG2 at YKTVMV
Reply-Bit: RLG2 at WATSON
Reply-Xin: rlg2@ibm.com

Jim, I will not be in Santa Fe the first day (Tuesday), when Integrated Electrochemical Experiments are scheduled for presentation. My Abstract will be published in the book, but I will present the paper at a Poster Session.

The poster will be 4 ft wide by 5 ft high. Your 10 pages of print are printed too small for legibility.

1. Please reprint with larger font (present point value multiplied by 1.8 should be fine) so that I can provide a stack of preprints.

2. Please reprint with MUCH larger font so that the actual poster is legible. Please print in "landscape format" so that you occupy 4x6 or 24 pages. I will post 4 pages wide by 6 high, which will still leave room for the existing 3 figures. Right now, the font is too small in respect to the line spacing. Reprinting as (1) will not reduce the number of lines on the page, but it will widen the letters and expand to about 16 pages. Going to font about 1.3 time LARGER than that (about 2.2 times present font, but not expanding the space between lines in proportion) should give me my 24 pages.

I leave the lab Tuesday afternoon for Santa Fe.

Thanks.

Dick Garwin

PRBIT SCRIPT Q1 dated 89/05/18 21:47:21 ..... Page 1

From: mmb@clockwise.att.com
Date: Thu, 18 May 16:29:50 1989
To: arpa!rlg2@ibm.com
Subject: Santa Fe meeting

Dick: I just got your message re your 10 min. presentation at the Wedn. evening discussion: I would enjoy that very much and will encorporate it into the program.

I also just read in today's WSJ that our duo Pons/Fleischmann will not be there: is there anything that you (I believe that you're on the DOE committee) can do about this through your LANL contacts?

regards,

Thijs Broer
(201) 582-7761
FAX: (201) 582-2913
BITNET: mmb%clockwise@att.com

Reed Jensen
(505) 667-1233

PRBIT SCRIPT Q1 dated 89/05/18 21:54:59 ..... Page 1

OPTIONS: NOACK LOG LONG NOTEBOOK *
Local options: Search RealNode

Date: 18 May 1989, 21:50:44 EDT
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
P.O. Box 218
Yorktown Hts, NY 10598
To: mmb%clockwise.att.com at relay.cs.net
Subject: Santa Fe mtg.
Reply-To: RLG2 at YKTVMV
Reply-Bit: RLG2 at WATSON
Reply-Xin: rlg2@ibm.com

I'll call LANL tomorrow to see what I can do. Thanks for planning to give me time Wednesday evening.

Dick Garwin

Problems with the γ-ray spectrum in the Fleischmann *et al.* experiments

SIR—Fleischmann, Pons and Hawkins[1] recently announced the observation of significant heating in their cold-fusion experiments, a result that they attribute to copious fusion reactions. As compelling evidence that fusion had occurred, they reported the observation of the 2.22-MeV γ-ray line that originates from neutron capture by hydrogen nuclei[2,3]

$$n + p \rightarrow d + \gamma\ (2.22\ \text{MeV}) \qquad (1)$$

(Here d represents a deuteron.) They contend that the neutron in reaction (1) is generated by the reaction

$$d + d \rightarrow n + {}^3\text{He} \qquad (2)$$

and conclude, therefore, first that the 2.22-MeV γ-ray confirms that the fusion process (2) is occurring, and second that a neutron production rate of the order of 4×10^4 neutrons s^{-1} is derivable from their γ-ray signal rate. They further state that most of the heat generation occurs not through process (2), but through a hitherto unknown nuclear-fusion process.

Here we focus solely on the identity of the reported γ-ray line, which we shall henceforth call the signal line. We argue that the claim of Fleischmann *et al.* to have observed the 2.22-MeV line characteristic of reaction (1) is unfounded. We do so on the basis of three quantitative considerations: (1) that the linewidth is a factor of two smaller than their instrumental resolution would allow; (2) that a clearly defined Compton edge[4], which should be evident in their published data at 1.99 MeV, is not in fact present; and (3) that their estimated neutron production rate is too large by a factor of 50. In addition, from a consideration of the terrestrial γ-ray background, we argue that their purported γ-ray line actually resides at 2.5 MeV rather than 2.22 MeV. These conclusions are, in part, based on our studies of neutron capture by hydrogen, using a neutron source submerged in water. These measurements allow us to compare the results of Fleischmann *et al.* directly with a controlled experiment.

We measured terrestrial γ-ray background spectra in order to compare our detector characteristics with those of Fleischmann *et al.* Figure 1*a* shows a typical terrestrial γ-ray background spectrum obtained with a 3 in. × 3 in. NaI(Tl) crystal spectrometer system (see ref. 5 for details). The main features of the background spectrum are quite similar throughout the terrestrial environment[6,7]. Fleischmann *et al.* showed a similar γ-ray spectrum on television (Fig. 1*b*). (We believe that we have viewed all the cold-fusion γ-ray spectra that have been shown on KSL-TV (Utah) up to 19 April. This information was obtained from Utah News Clips, Inc., Utah. As far as we can tell, all spectra are identical to that of Fig. 1*b*.) This spectrum was obtained in the course of the Fleischmann *et al.* experiments (M. Hawkins, personal communication). Their spectrometer system consisted of a Nuclear Data ND-6 portable analyser with a 3 in. × 3 in. NaI(Tl) crystal (ref. 1 and M. Hawkins and R. Hoffmann, personal communications). A 3/8-in.-thick Pb annulus encompassed the scintillator. It is clear from Fig. 1*a* and *b*, and particularly from the ^{40}K (1.46 MeV) and ^{208}Tl (2.61 MeV) lines, that our resolution is comparable to or better than that of the spectrometer used by Fleischmann *et al.*, a point to which we return later.

In the interval 1.46–2.61 MeV, the energy resolution of a NaI(Tl) spectrometer, which determines the γ-ray linewidth, can be well described by the formula[8,9]

$$R(E) = \frac{\Delta E}{E} \approx R(E_n)\sqrt{E_n/E} \qquad (3)$$

Here ΔE is the full width at half maximum (FWHM) of the line, E is the energy of the photon and $R(E_n)$ is the measured 'reference' resolution at energy E_n. $R(E_n)$ can be accurately determined using a ^{60}Co source (that is, the ^{60}Co line at 1.33 MeV), or it can be fairly well approximated by the ^{40}K decay line at 1.46 MeV. (From Fig. 1*b*, the ^{40}K decay line allows one to estimate Fleischmann *et al.*'s resolution as ~ 8%.)

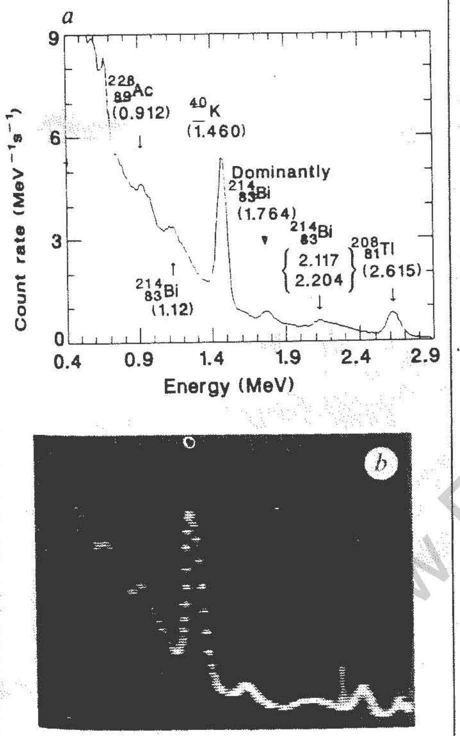


FIG. 1 *a*, The γ-ray background spectrum measured with a 3 in. × 3 in. NaI(Tl) detector at MIT. Some important terrestrial γ-ray lines have been identified in this figure[6,7,12]. (As explained in ref. 12, the immediate parent of the final decay product is identified. For example, ^{40}K β^+ decays into an excited nuclear state of ^{40}Ar, which actually then emits the 1.460-MeV photon discussed in the text.) The spectrum is averaged over an 84-hour run. *b*, The γ-ray spectrum shown on television by Fleischmann *et al.* The main characteristics of the two spectra are similar; one can also tell that the two detectors have comparable spectral resolution. In *b*, note the curious structure at about 2.5 MeV and that beyond the ^{208}Tl peak (2.61 MeV), which appear to be artefacts. (The spectrum can also be obtained from KSL-TV in Utah (M. Hawkins, personal communication).)

TABLE 1 Comparison of energy resolutions of the γ-ray spectrometers

(*a*) *Resolution of MIT spectrometers*

Energy (MeV)	1.17	1.33	1.46	2.22	2.61
Origin	^{60}Co	^{60}Co	^{40}K	n(p,γ)d	^{208}Tl
Natural background			0.055		0.043 (0.041)
^{60}Co	0.056	0.051			
Pu/Be neutron source				0.05 (0.045)	

(*b*) Resolution of the Fleischmann *et al.* spectrometer[1]

Energy (MeV)	1.33	1.46	2.22	2.61
Reference	^{60}Co	^{40}K	n(p,γ)d	^{208}Tl
Hoffman*	0.056	0.065		
TV news†		~0.08		~0.05 (0.049)
Ref. 1 (errata)			0.025 (0.053)	

The resolution is defined as the full width at half maximum (FWHM) divided by the peak energy. Numbers in parentheses are predicted values based on the detector resolution at 1.46 MeV (see text). In *b*, the prediction is based on the resolution value (0.065 at 1.46 MeV) provided by R. Hoffman (personal communication).
*R. Hoffman (personal communication).
†Derived from images of the televised news broadcasts.

Table 1 lists the resolution data for our detectors and for that of Fleischmann *et al.*.

We now compare the signal line of Fleischmann *et al.* (Fig. 1*a* of ref. 1 (errata), shown as Fig. 2 here) with our measured spectrum obtained from the experiments on neutron capture by hydrogen (Fig. 3 here, and Fig. 4 of ref. 5). In these experiments, a Pu/Be neutron source was placed in a water tank. ${}^{239}_{94}$Pu emits energetic α-particles, which produce neutrons through (α, n) reactions with Be (refs 4,9). The neutrons are thermalized in water, and we observe the emitted neutron-capture γ-rays with our spectrometers. The measured resolution at 2.22 MeV is ~ 5% (Table 1*a*), and is reasonably well predicted by equation (3). As a consequence, this calls into immediate question the identity of Fleischmann *et al.*'s signal line as a γ-ray line. Specifically, Fig. 2 shows the signal line to have a resolution of 2.5%. This is about a factor of two smaller than that predicted by equation (3) on the basis of the known resolution (Table 1*b*) from either the ^{40}K decay line (1.46 MeV) or from the ^{60}Co source (1.33 MeV) (R. Hoffman, personal communication). But we know from Table 1 that the spectrometer used by Fleischmann *et al.* has a resolution that is at best comparable to our own for the entire region from 1.46 to 2.61 MeV (see also Fig. 1), so it is inconsistent that their linewidth at 2.22 MeV is a factor of two below the predicted value.

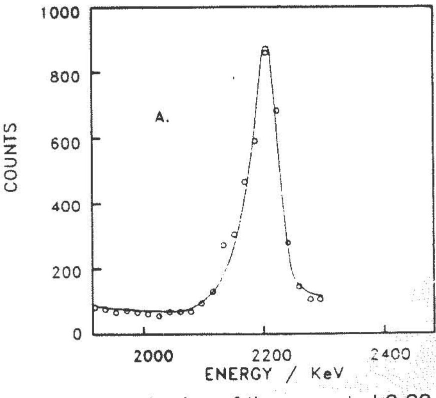


FIG. 2 A reproduction of the purported 2.22-MeV γ-ray signal line of Fleischmann *et al.* (Fig. 1*a* of errata to ref. 1). The resolution, based on the linewidth, is about 2.5%. With such resolution, one would expect to see a clearly defined Compton edge at 1.99 MeV. No edge is evident. Also, a resolution of 2.5% is inconsistent with their spectral resolution. Furthermore, we argue that the signal line may reside at 2.5 MeV, not at 2.22 MeV as is claimed by Fleischmann *et al.* and depicted here.

There is a second crucial inconsistency with the published signal line (Fig. 2). If we assume a resolution of 2.5% at 2.22 MeV, then there should be a clearly defined Compton edge[4] at 1.99 MeV. For example, in Fig. 3 the Compton edge is evident even for our measured resolution of only 5%. For a resolution of 2.5%, the definition of the Compton edge would be distinctly sharper. The lack of a Compton edge at 1.99 MeV for the signal line therefore negates the conclusion of Fleischmann *et al.* that they have observed the 2.22-MeV γ-rays from neutron capture by hydrogen.

We also point out that in our (Pu/Be) neutron-capture experiments, a conspicuous e^+-e^- annihilation single-escape peak exists at 1.71 MeV (Fig. 3), as well as a double-escape peak at 1.20 MeV. (The full spectrum from the Pu/Be experiment, as well as the background spectrum, can be found in ref. 5.) Such features unambiguously identify the primary γ-rays as having an energy of 2.22 MeV, and are a necessary consequence of the physical processes of detection of γ-rays in a finite-sized NaI scintillator.

Based independently on both their γ-ray and neutron measurements, Fleischmann *et al.* claim to have observed a neutron production rate of ~ 4×10^4 neutrons s^{-1} (ref. 1). This claim is clearly inconsistent with their γ-ray signal line, for the following quantitative reasons. The Pu/Be neutron source used in our experiment is absolutely calibrated to within 10% of 1.5×10^6 neutrons s^{-1} (ref. 10 and MIT Reactor Radiation Protection Office). In obtaining the data in Fig. 3, we used an experimental setup similar to that of Fleischmann *et al.* (ref. 1; televised broadcasts; and M. Hawkins and R. Hoffman, personal communications). Our Pu/Be source was submerged 6 in. into a large water tank. The rate at the 2.22-MeV peak, after subtracting the background continuum, is about 1.4×10^3 MeV^{-1} s^{-1} (see Fig. 3). Scaling this rate to a neutron source of 4×10^4 neutrons s^{-1} (the level given by Fleischmann *et al.*), and integrating over the linewidth, gives a total 2.22-MeV γ-ray rate of about 4.5 counts per second. This value is a factor of 50 times higher than the rate that would be calculated on the basis of the results in Fig. 2 ~~1a~~ (that is, 0.081 counts per second). (Fleischmann *et al.* state that their neutron count rate is measured with a BF_3 neutron counter over a 0.4 mm × 10 cm Pd cell, and that the γ-ray measurement is over a 0.8 mm × 10 cm Pd cell[1]. If the total reaction rate is proportional to the volume of Pd rod, as they state, the inconsistency in the reported neutron rate is by a factor of 200 rather than 50.) While differences in rates of a factor of two might possibly be explained by geometrical considerations, a factor of 50 is inexplicable.

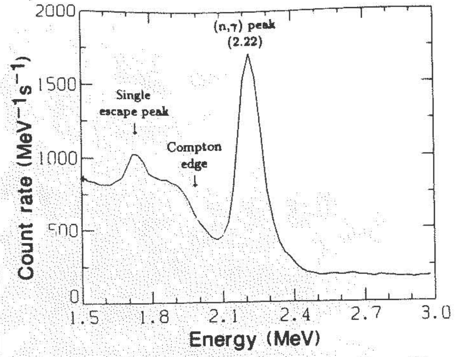


FIG. 3 The γ-ray spectrum measured by a 3in. × 3in. NaI(Tl) spectrometer during a neutron-capture-by-hydrogen experiment using a (Pu/Be) neutron source submerged in water. Because of the finite size of the crystal (which is identical to that of Fleischmann *et al.*[1]), we also see an escape peak[2-4] and, of particular importance here, the Compton edge[4]. In this figure, the digitization energy width is 0.024 MeV per channel. The full Pu/Be and background spectra are shown in Fig. 4 of ref. 5.

A further point concerning the identification of the signal line is the precise value of the energy at which the peak occurred. From Fig. 2, the background in the neighbourhood of the peak is seen to be ~ 80 counts per channel, a level that corresponds to ~ 400 counts per channel for a 48-hour accumulation time (the data in Fig. 2 were accumulated for a period of 10 hours[1]). On the other hand, in the Utah measurements of terrestrial γ-ray background, the level in the vicinity of the 2.22-MeV feature was found to be ~ 4,000 counts per channel (R. Hoffman, personal communication). The only relevant part of the entire γ-ray spectrum (between 1.46 and 2.61 MeV) in which the background was as low as 400 counts was at an energy in the vicinity of 2.5 MeV (R. Hoffman, personal communication). Thus, we argue that the peak in the spectrum shown in Fig. 2 may be at 2.5 MeV, not at 2.22 MeV.

The importance of properly identifying the energy of the feature claimed by Fleischmann *et al.* can hardly be overemphasized. Thus, it is extremely unfortunate that they chose to display only the energy range 1.9–2.3 MeV in their pub- ▶

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ACKNOWLEDGEMENTS. We thank C. ~~Y.~~ Kurz, J. S. Machuzak, F. F. McWilliams and Dr S. C. Luckhardt. For the use of a spectrometer system, we thank Professor G. W. Clark. For discussions, we thank M. Hawkins and R. Hoffman of the University of Utah. For suggestions and criticisms, we are grateful to Dr G. R. Ricker Jr and Professor D. J. Sigmar. We are indebted to J. K. Anderson for assembling this document. For locating important references, we thank K. A. Powers. Supported in part by the US Department of Energy.

lished Fig. 1*a*, thereby not providing the supporting evidence of the ^{40}K (1.46-MeV) and ^{208}Tl (2.61-MeV) features which must be present in their spectra in order for their identification to be correct.

Therefore, although Fleischmann *et al.* may have observed a change in their γ-ray spectra that bears some relation to detector location, we conclude that it is unrelated to the 2.22-MeV neutron-capture γ-rays, and that it is also unrelated to the background $^{214}_{83}$Bi line (2.20 MeV; Fig. 1*a*), as has been suggested elsewhere[11]. We can offer no plausible explanation for the feature other than it is possibly an instrumental artefact, with no relation to a γ-ray interaction.

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Fusion in from the cold?

Sir—Recent experiments involving the loading of deuterium (D) into palladium[1] via a 0.1 M LiOD/D_2O electrolyte may have created something not encountered in the absence of current flow. The voluminous body of older work on electrolytic D–Pd loading is based on thin wires or foils of high aspect ratio which allow only small voltage differentials. But the reported chemical potential across a thick deuterated palladium negative elctrode could give rise to metastable solid phases alien to the existing equilibrium Pd–D phase diagram. The theoretical existence of palladium analogues to hydrogen-rich[2] compounds like Li_2ReH_9 is as tantalizing as the limit of solid solubility of D in Pd is finite. And beyond PdD_3 (by analogy to the known compound Li_3Pd) the thermodynamic distinction between the higher palladium deuterides and metallic deuterium grows dim. The mechanism of their formation might involve the intersection of high deuteron mobility grain boundaries with the Cottrell clouds of H and D that decorate dislocations in many transition metals.

A small mass of (mostly) deuterons transforming into molecular deuterium as they recapture electrons previously delocalized into the S- and D-bands of palladium could release roughly 1 MJ mol^{-1}. Apart from explaining the electrode meltdown and vaporization reports[1] (although this could be due to lithium's remarkable ability to lower the melting point of palladium), this could raise local temperatures to very high values.

Could the so-called 'cold fusion' environment in fact involve local temperatures of greater than 10^5 K generated by the detonation of deuteron clusters (R. G. Gordon, personal communication) or of a metallurgical precipitate, coarse or fine-grained, of a high D/Pd ratio intermetallic compound inside a deuterated palladium electrode? The energy of reassociation of electrons and deuterons is roughly 1 Rydberg (13.6 eV) minus the work-function of Pd (4.9 eV); to this must be added the roughly 4.7 eV liberated when two deuterium atoms pair. Obviously, these energies must be scaled down to compensate for the difference between reactions in free space and the solid state. Nevertheless, the 20 eV energy of formation of D_2 from deuterons could thus produce hot and highly compressed deuterium plasma bubbles of small (>0.01μm–<100μm) size. As to the objection that the surrounding cool metal will quench these bits of pale fire in a nanosecond or so, I believe it answers a serious question: how come the Fleischmann and Pons[1] claimed neutron yield is 9–14 orders of magnitude short of their claimed heat flux? At present, one can only speculate as to by how many orders of magnitude a reflected spherical shock front might raise the temperature and pressure of the D-plasma.

There is another ramification to the notion of nano-novas flashing out of perdeuteride grains decorating the grain boundaries of electrochemically overwrought palladium nagative electrodes. The explosion of such precipitated grains will, above a critical radius, generate cracks in the adjacent metal. It has been postulated (G. Chapline, personal communication) that the surfaces of such cracks, as they open, could host a field gradient or a propagating array of plasmons down whose wake field a deuteron could accelerate. This presumes that the bulk Pd–D system has reached 1:1 stoichiometry and thus been restored to long-range order; this condition is very far from the disorderly and anharmonic state of Pd–D during the early stages of electrolytic D-loading. In this model some thousands of unit cells of travel would suffice to yield keV deuterons. So the propitiated shade of Rutherford may yet countenance not-so-cold fusion.

It has been noted in a News and Views article in the 27 April issue[3] that neutrons have been observed when cracks are generated in crystals of lithium deuteride[4].

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Mössbauer cancer therapy doubts

Sir—Mills *et al.*[1] present data to support the view that a dose of 10^{-5} Gy of 14.4-keV X-rays can ablate a population of malignant cells containing ^{57}Fe(III) · bleomycin. They suggest that such a regime may have the potential for the low-dose sterilization of superficial human tumours.

This is unlikely on simple physical grounds, basically because only a small fraction of the exposed cells will have received any energy deposition at all. The proportion of cells that would receive one or more energy depositions, assuming the statistical independence of such events, is obtained from the Poisson distribution and is $(1 - e^{-n})$; here n, the average number of energy depositions occurring in the sensitive site, is given by D/z_{1F}, where D is the absorbed dose and z_{1F} is the so-called 'frequency-averaged specific energy per event' — or simply, the mean energy per unit mass deposited by single energy deposition events in the sensitive site in the cell[2]. This mean specific energy will be similar, irrespective of whether the photon is ultimately absorbed in a photoelectric event or in a Mössbauer absorption (it would be slightly larger in the latter case); it has been measured for 12-keV photons in a tissue-equivalent material in spherical sites of various volumes[3].

Appropriate volumes for consideration are those of typical human cell nuclei (100–1,000 μm^3)[4] or, perhaps more relevantly, the volume of nucleotides in the mammalian nucleus (~3 μm^3)[5]. For volumes of 250 and 3.5 μm^3, for which measurements have been made[3], the corresponding values of z_{1F} are 4×10^{-3} Gy and 0.14 Gy. These numbers yield a probability of about 2.5×10^{-3} that a 250 μm^3 volume of cell nuclei will be subject to at least one energy deposition, and a corresponding probability of 7×10^{-5} for a 3.5 μm^3 volume of nucleotides. Thus, only about 1 in 400 cells (250 μm^3 volume) or 1 in 14,000 cells (3.5 μm^3 volume) would receive any energy deposition at all if exposed to a dose of 10^{-5} Gy.

To sterilize even a small tumour containing about 10^8 cells requires an appropriately small probability (less than 10^{-9}) that any cell will receive no energy-deposition events. Thus, an average number of energy depositions per cell of

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Dear E632 and WA84 Colleagues,

18 May 1989.

COLD FUSION NEWS No. 13

by D.R.O. Morrison, CERN

For RLG
Cold Nuclear Fusion
File

Introduction

1. NEW RESULTS, THEORY AND REGIONALISATION OF RESULTS
   1.A Calorimetry
   1.B Fusion products
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INTRODUCTION

After the American Physical Society meeting in Baltimore, 1 and 2 May, it seemed to most present that the four original experiments of Fleischmann and Pons were mistaken. However since then there has been the meeting of the Electrochemical Society in Los Angeles on Monday 8 May where despite an absence of details, they had some media success and some new confirmations. Also the results that I have received tend to be more confirmations than null results, in particular there is a preprint from the Bologna - Brigham Young Collaboration in the Gran Sasso tunnel which confirms the result of Jones et al. These new results will be reported and discussed. Thus the question may not be settled until the Workshop on Cold Fusion to be held at Santa Fe from 23 to 25 May. It is sponsored by Los Alamos and DOE - and since Fleischmann and Pons have announced that they are collaborating with Los Alamos only, they can hardly avoid turning up without losing all scientific credibility. This could be a decisive meeting.

This note is in two distinct parts;

1. A presentation of the situation and the new results. Evidence is given for the regionalistion of results. This part will be essentially conventional science.
2. However it is felt that Cold Fusion cannot be understood purely from the results provided by the different scientific groups, as is shown by the evidence for the regionalistion of results which is contrary to our basic belief that the Laws of Science are universal and should be the same in all parts of the world. Hence the second part gives some background that may help to explain the observed regionalisation of results and the disagreements between the results. The subject of Cold Fusion is discussed in terms of Pathological Science and some historical perspective is given.

Here we will again emphasis that there are two main experiments in the so-called Cold Fusion subject, and these should be treated separately. First there is Calorimetry and secondly observation of fusion products.

A) Calorimetry

Claims have been made of unusual heat production, and Fleischmann and Pons say this can be about 10 Watts per cm3 of Palladium. The origin(chemistry, fusion, ...) of this power is not established though people would expect it to be fusion. It is this experiment by Fleischmann and Pons which raised world-wide the hope of unlimited power from water with little attendant pollution.

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B) Fusion Products

Claims of observation of neutrons, gammas, tritium and helium as evidence of fusion have been made, but the corresponding power is very much less, about 1 E-9 to 1 E-13 watts which would be much less than the power put in and completely useless for power production.

The important point is that up till now none of the observations claimed of fusion products has supported the hope from the calorimetric results that economically useful power can be produced.

1. NEW RESULTS, THEORY AND REGIONALISION OF RESULTS

1.A. CALORIMETRY

At the Los Angeles meeting of the Electrochemical Society which was attended by some 2000 people, there was a special session on Cold Fusion. What follows is from many different sources (unfortunately none of them scientific papers!), so is liable to error.

Fleischmann and Pons had said they would produce new results but there was some disappointment as they not give any more details of their experiment or new results except to claim that they now had much more power, about 10 to 50 times more than they had put in (previously was 4 to 8 times input). But no detailed justification was presented. According to AP they observed this energy as a burst of heat that lasted two days. Previously Pons had been reported as saying that when they tried normal water instead of D20, they had obtained excess heat also. However the report by Nature quotes Fleischmann as saying 'any suggestions that control experiments done with ordinary water were yielding excess heat comparable to that produced with heavy water were "total nonsense"'.

Possibly the best summary of what they said is in the Utah paper, the Deseret News;

"Pons and Fleischmann reported;

-- experiments continue to show large releases of heat
-- total watts put into the cell are less than the total energy output (without considering the electrochemical reaction)
-- energy coming out of the system is fairly constant, but in some situations there are large bursts of energy
-- The bursts are enormous and, if persistant, are capable of literally boiling the cell out at a very low voltage
-- one cell, running at 32 degrees for 5 1/2 million seconds {about two months} suddenly burst up to 60 degrees and remained at that temperature for several hours
-- bursts of neutrons and other radioactive particles, usually ignored by physicists, have been seen
-- some bursts have lasted long enough to enable scientists to go into the machine to check instruments
-- the heat output from the sustained bursts over a two day period have been between 1000 and 5000 percent more than the input."

[comment - there seems to be more information here than in any other report that I have read on the Electrochemical meeting. Also this is the first time that I have seen a value of temperature quoted (there is none in their published paper)].

At this meeting they were supported by Huggins et al. of Stanford who said that he had observed excess heat with coin-shaped palladium rods which were totally submerged. He assumed that there was no recombination of gases. When H20 was added to reduce the concentration of D20, the heat generation stopped. He said that the cell with D20 ran consistently hotter than a H20 one.

Dr. Landau of Case Western Reserve claimed to have observed 40% more excess heat than Fleischmann and Pons had reported with D20. He said there was no heat generated above error with H20. He reported no recombination of D2 and 02 to 3% accuracy.

Two groups from Texas A and M, Appleby et al. and Bockris et al., reported excess heat. {Earlier it was written in New Technology Week, that the University had devoted 30 of their 70 electrochemists to replicating F&P's experiments. They used a 0.1 cm diameter rod which was 5 cm long. Packham said they "detected a 60 to 70% heat excess" compared to the current input to the electrode of 60 - 100 mA per cm2. The experiment produced a 3 to 4 degree difference in temperature with 4 watts of electrical input}.

[Comment - up to now the only quantitative estimate of excess heat expressed as watts per cm3 of Pd comes from Fleischmann and Pons - it would be good if the other groups would give values in watts per cm3 also].

A major criticism of the Cal Tech group of Lewis et al. and Meyerhof et al., was that there was no stirring in the electrolyte of the cells so that temperature gradients were present and this would mean that the temperature read would vary according to the position of the thermometer. Then since Newtons Law of cooling was used to establish the energy produced, this would affect the calculation of excess heat. This was answered by Fleischmann who showed a video of a cell bubbling energetically and when a dye was added to the liquid, it mixed in about 20 seconds. This was presented as evidence that there were no temperature gradients [comment - it would have been more decisive if the thermometer had been moved around or an array of thermometers had been used].

An even stronger argument against the effect of stirring comes from the Stanford Daily of Wed. 10 May, where Prof. Huggins is quoted as saying that "he has performed experiments in which he stirred the water at different rates demonstrating that the effect continues"

There was some discussion of the reproducibility of the results. All agreed it was not 100%. In their original paper, Fleischmann and Pons mentioned 5 cathodes of which 4 gave excess heat and the fifth was dead "because it gave no excess heat". Huggins suggested it had to be cast and not annealed and should be purged of hydrogen first, but Landau of Case Western Reserve Univ. said they had used annealed cathodes. Bockris said that only 5 or 6 out of 20 palladium electrodes had given excess heat. Lewis said they had used 50 different electrodes, some cast, some annealed and had observed no effect. Earlier in the Deseret News Pons was quoted as saying that from failures round the country, he had deduced the connection between cast Pd and success and milled Pd and failure. Originally they had often failed to generate heat, but now they can get working cells "90% of the time".

It was generally considered that if the excess heat were due to fusion but that the process was such that conmventional fusion products were not observed in sufficient quantities to explain the effect, then the only solution would be that helium was formed with energy and this energy was not emitted as gammas but converted into heat. This would mean that the palladium rods must contain a large amount of helium. Earlier Fleischmann and Pons had said that they had observed helium (Nature 338 (1989) 691) but at Los Angeles they said that this result was premature [other reports indicate that the problem concerns the amounts of background helium, as plagued the 1926 experiments of Paneth and Peters].

A group at the University of Florida, Gainsville, L. Muga et al. report that after several weeks running, they do not observe any abnormal heat effects.

A group at Harwell led by David Williams, is doing a large number of experiments and will not publish for another month. When interviewed by the Sunday Times, David said that no excess heat was observed. What is of special interest is that they are doing very careful experiments, in particular their calorimetric experiments will be at a moderately high constant temperature and then varying the amount of heat input to balance any heat excess or or deficit, which is a much safer technique than observing temperature rises and than using Newton's Law of cooling to deduce the amount of energy produced.

A paper has been distributed electronically by the Tata Institute for Fundamental Research, Bombay, by E. Krishnakumar, et al. entitled; "Calorimetric Studies of Electrolysis of D2O and H2O using a Palladium Cathode" It is unusual in this field in that it gives a detailed description of what was done. Two cells were run for a total of 200 hours, one with D2O and the other with H2O. They were run at four successive current densities, firstly at 25 and 24 mA cm-2 for D2O and H2O resp., the difference being to keep the power levels equal at 0.06 watts. Secondly at 50 and 52 mA giving powers of 0.170 and 0.172 W resp. Thirdly with 100 and 110 mA giving 0.43 and 0.42 W resp. and finally with 200 and 210 mA resp. (power not given). They observe that the D20 cell has a higher temperature than the H20 cell for phases 2, 3, and 4. being about 2 centigrade degrees difference. Care had to be taken to maintain the levels of the electrolyte constant as when this was not done, differences were found. They say that they did not find dramatic effects as others had done (one reference to this is K.S.V. Santhaman et al. Indian J. of Tech. 27 (1989) 175). The paper is said to contain what I have long asked for, that is graphs of the variation of temperature with time - but unfortunately the electronic transmission means that some words are lost and more importantly, the interesting graphs!

[Comments.

(1) The major comment of Lewis et al. was not answered. This was the taking the published numbers of Fleischmann and Pons and making assumptions based on detective work, to calculate the power in and the power out. It was found that the cell is acting as a refrigerator which is not cooling down as much as expected. This lack of cooling down is treated by F&P as excess heat produced and who then by a series of assumptions and extrapolations derive a large power output of many Watts. So far I have not seen any denial of the numbers used by Lewis or refutation of his method - but maybe these were made but not repeated in Nature or the press or people present.

(2) The "confirmations" of excess heat of Stanford, Texas A&M (twice) and of Case Western Reserve seem to derive from an observation that the Pd electrode in the D20 ran at a higher temperature than the one in H20 as is described in detail by the Tata Institute group. If one assumes this difference is a proof of excess heat, then one could calculate this as Watts per gm of Palldium. However it does not seem to answer the argument of Lewis et al. To answer it one needs a full account of all the energy flows, assumptions, voltages and the actual calculation. So far I have not heard of such a presentation. Better still would be a good calorimetric experiment with a closed system and the apparatus kept at constant temperature.]

1.B. FUSION PRODUCTS

At the Electrochemical conference, Prof. Fleischmann said that they realised that the gamma ray line that they claimed to have observed at 2.2 MeV was too narrow and too weak to be from neutron capture. [this was generally taken as a retraction and follows criticism that their counter did not have enough resolution to give such a narrow peak - this is apart from worries about background from natural sources such as radon or thorium decays products.

There have been several reports of the observation of fusion products by Southern European groups, but in recent conversations, the repetition of some experiments are meeting problems of reproducibility.

However there is an important new preprint from an experiment in the Gran Sasso tunnel where the background from Cosmic Rays is expected to be very low as they are under 4000 metres of water equivalent. This experiment is being performed by the Bertin et al. group of the Univ. of Bologna and the INFN, Bologna together with the Brigham Young group of Jones et al. Typically three cells were operated simultaneously with cathodes of fused titanium pellets of about 1 gram. The DC power supply was 6-10 Volts and the currents 40 to 60 mA. The 2 neutron counters were with NE213 scintillator liquid and n-gamma discrimination was obtained by gating fast and slow pulses. The cells were kept in front of counter A while the other counter B was at 8 metres. "After about one hour following the connection of the cells to the power supply, the counting rate defined this way, markedly increased, went through a maximum and eventually dropped (in about three hours) at the level of the background counts." This was observed twice more. The raw data are not shown, but the background-subtracted data are given and show a neutron energy distribution which agrees very well with the Monte Carlo expectation for 2.45 MeV neutrons and corresponds to 350 +/- 51 neutrons. It looks impressive, but some comments can be made;

1. It is written more than once that the results are preliminary. Now much of the trouble with the subject of cold fusion is groups publishing or presenting preliminary results - at this stage it is not necessary. We need solid results.
2. The total cell operation period was 10 hours. This is very short for an important result. A few days more would have allowed time for basic controls that have not been done.
3. Fleischmann and Pons were often criticised for placing their calibration counter at a distance - here counter B is placed at 8 metres. It has been repeatedly said that the correct way is to intersperse controls e.g. to run with H2O, and also to run with a platinium cathode instead of a titanium one. In view of the relatively high statistics claimed, it should be possible to do all of these in a few days.

Another experiment has been done deep underground. This is by the Bugey - Grenoble Collaboration that had previously found no neutrons using an exceptionally good detector. They measured neutrons again but with a less sophisticated detector, but still a good one, in the Frejus tunnel. Yves Declais told me that their cosmic ray induced background is about one per year, but due to natural radioactivity, their actual background is 0.05 neutrons/hour or about one a day. They tried a number of different ways of inducing cold fusion including electrolysis and the techniques used by Prof. Scaramuzzi of Frascati of using pressure to look for non-equilibrium effects. They found no increase in the number of neutrons recorded. Thus this experiment is in contradiction with the results of Frascati, Gran Sasso and the original results of Jones et al. Yves will present these results at the Los Alamos meeting next week.

There was a report by press conference, from the University of Florida, Gainsville, of the observation of tritium production from a cell. This was interpreted by the authors, two emeritus professors of nuclear engineering, as confirming Cold Fusion. However another group at Gainsville, L. Muga et al., comprising nuclear chemists, condensed matter physicists and nuclear physicists, do not find any tritium production after running for several weeks - they checked carefully for tritium contamination. Also Muga et al. do not observe neutrons or gammas [unfortunately I do not have numerical limits].

A paper "Deuterium Nuclear Fusion in Metals at Room Temperature" by P. Thomas et al. of the "Ruder Boskovic' Institute, Zagreb, Yugoslavia, reports on neutron measurements from a three-compartmental electrolytic cell. The Palladium cathode was surrounded by a platinium wire anode but it was found after a time that the anode had dissolved. Using X-ray fluorescence spectroscopy it was found that there was a thin platinium layer on the palladium cathode. As this layer might hinder the passage of deuterium into the palladium, a large palladium sheet was then used as the cathode. A 6Li-glass scintillator was used as a neutron detector. Using a 252 Cf source its efficiency was found to be 1%. The total counting rate over all channels was 0.15 impulses/sec. The counting rate for neutrons was 0.01 per sec. The experiment lasted 3 E5 seconds. The amount of deuterium in the palladium was checked by weighing. Typical values of 0.15 grams of deuterium per 14 grams of palladium were found. No significant deviation was observed, this gave an upper limit of 0.01 neutrons per gram of palladium. In preliminary experiments using titanium powder under pressure and cooled to liquid nitrogen temperatures, it was found that the cooling affected the stability of the neutron detector [and no results were given].

Prof. Leonid Ponomarev has informed me of several experiments being performed at the I. V. Kuratchov Institute of Atomic Energy using Pd and Pt electrodes and D20 and LiOD electrolyte. The sorption and desorbtion of deuterium was studied at different temperatures and pressures(up to 1.5 Kg). The counters for neutrons were scintillators, BF3, and 3He while for gammas a NaI counter was used. No gammas were found. For neutrons the result was (1 +/- 1) E-3 neutrons per sec per gram of Pd which is much less than reported by Fleischmann and Pons and less than Jones et al.

[Have been told of several other experiments all of whom have null results but for various reasons, do not wish to publish - one experimenter summed it up as "no neutrons, no fusion, and no publication"].

1.C. THEORY

The famous chemist Linus Pauling has had a letter published in Nature [as he is 88 years old, is this a record?]. He recalls a paper in Physical Review that he wrote in 1938 where he pointed out that in transition metals, 0.72 orbital per atom of the orbitals in the outer shell remains apparently unused. He identified the 0.72 orbital per atom as the metallic orbital which gives metallic properties. He noted that this could explain why palladium is saturated with hydrogen at the composition PdH(0.6). He judges that in F&P's experiment, the deuterium is being forced into forming an unstable higher deuteride such as PdH(2). "The subsequent decomposition of which would give an increased evolution of heat or to the explosion of the deuteride into palladium powder and hydrogen gas".

The language that Pauling uses is different from those of papers I have read recently and is a useful complement giving a return to some fundamentals. It is considered that the octahedral sites are occupied first then more tetrahedral ones. For PdH(x) where x is about 0.6 to 0.7, there are some phase changes in the system, the exact value depending on temperature. These effects can be studied by neutron scattering or by implanting polarised muons. Keith Ross of Birmingham has given me the following references;

"Implanted muon study of supperlattice ordering in palladium hydride PDH(0.64)" by S.F.J. Cox et al. Hyperfine Interactions 31 (1986) 105 - 111

"Interatomic potentials and lattice distortions in PdD(0.8)" by M.W. McKergow et al. Zeits. fur Phys. Chemie neue folge Bd. 146.S (1985) 159 - 169.

Incidently he also gave me an interesting paper on "A proposed method of hydrogen isotope separation using palladium alloy membranes" by J. Evans et al. Journal of Less Common Metals, 89 (1983) 407 - 414. The idea would be to separate hydrogen, deuterium and tritium in a fusion reactor (he means a hot fusion reactor!) This work may have some relevence to reports on the identification of tritium from cells.

[A point I would like to make again is that there is a tremendous literature on palladium hydrides and deuterides and it is surprising that people can think of the subject for years and not go to libraries.] Prof. Ponomarov send me an extract from the famous paper of S.S. Gerstein and Ya Zeldovitch, Sov. J. Uspekchi 61 (1960) where they talked of possible fusion 39 years ago;

"At first glance it seems that the necessary reduction in distance between the hydrogen nuclei may occur in certain chemical compounds; actually however in all known compounds containing hydrogen the distance between the hydrogen nuclei is greater than in the H2 molecule. Thus, interatomic separations in ordinary molecules are too large for the yield of sub-barrier nuclear reactions between hydrogen isotopes to have any significant magnitude".

In support of this, let me repeat the appropriate separations for Palladium;

palladium-palladium in metal	3.89 A
palladium-palladium in PdH(x)	4.03 A
deuterium-deuterium in octahedral sites	2.85 A
deuterium-deuterium in octahedral -tetrahedral sites	1.74 A
deuterium-deuterium in free gas molecule	0.74 A

(information from Prof. Pui Lam of Hawaii).

Nature also published a letter from J Gittus (UKAEA) and J. Bockris (Texas A&M) who presented a positive result on calorimetry at Los Angeles. They say that it is well known that hydrogen tends to segregate in cracks, grain boundaries or interfaces, and dislocations. They suggest "prior accumulation of hydrogen in Pd may prevent the build up of deuterium and this could explain why cold fusion is so hard to replicate". "When the concentration (of deuterium) in these local regions has reached a high enough level, the reaction commences in the segregated region. We are unable to say just what the reaction is, but it may be a form of nuclear fusion or some chemical reaction." "If this hypothesis is correct, it accounts for the failure of some laboratories to produce the 'fusion' reactions described by Fleischmann and Pons". [comment, both the Yale - BNL and Cal Tech experiments purified their Pd electrodes before starting, so their observation of no fusion cannot be described as a "failure"].

Another interesting hypothesis is "Scenario for energy production from cold fusion by free quark catalysis" by G.L.Shaw et al. They "present a scenario in which small numbers of free stable ubar-ubar = Q anti-diquarks (electric charge of -4/3, mass of a few GeV, and short range strong repulsion with hadrons) catalyse the deuteron fusion. The reaction channel He4 + Q dominates". They say 100 Q's are enough to give watts of power and "although no quark search has been made using Pd, Fairbank and collaborators found evidence for free quarks on little spheres of the transition metal niobium at a level as large as 1 quark per 1 E19 Nb molecules". However they predict neutrons at much higher rates than have been observed.

1.D REGIONALISATION OF RESULTS

At the APS meeting in Baltimore, I presented tables of results to justify the conclusion that confirmation and non-confirmation of the five positive results (calorimetry, neutrons, gammas, tritium, and helium) claimed by Fleischmann and Pons, were not uniformly distributed throughout the world, but rather confirmations occurred in certain regions and null findings in other regions. A full update will be given at the Los Alamos Workshop, and here will give a summary.

	CONFIRMATION		CALORIMETRY		FUSION PRODUCTS	
	Yes	No	Yes	Null	Yes	Null
NORTH-WEST EUROPE	0	11	0	3	0	8
USA REGION 1	1	10	1	1	0	14
USA REGION 2	10 - 1	1	6	0	7 - 2	0
SOUTHERN EUROPE	5	0	0	0	5	0
EASTERN EUROPE	6	2	2	0	5	3
REST OF WORLD	8	2	3 + ?	2	4	4

Some comments are required;

1) The information is not always easy to obtain or evaluate, therefore is liable to some error, but not enough to alter the conclusions. For example I have heard that in Southern Europe, there are 9 groups that find positive effects, not 5, but I have inadequate information on four of them.

2) The negative signs in the table are retractions (10 - 1, the one is Georgia Tech),(7 - 2, the two are Fleischmann's comments on their gamma and helium measurements).

3) The ? is for the Tata Institute's finding that the cathode runs hotter with D2O than with H2O - they do not draw any conclusions about this being a heat excess.

4) The United States is devided into two regions;

USA REGION 1 comprises that part of the country that is well informed by the New York Times style of science writing that thinks and asks before it publishes, plus the major research centres. The one positive finding is Prof Huggins at Stanford. I know of several other non-confirmations but the authors do not wish to publish or announce it.

USA REGION 2 comprises the rest of the country.

5) Each fusion product result is counted once, e.g. a group that it did not find neutrons nor gammas would be counted as two null results.

6) Since the APS meeting, there have been two null results in The Rest of the World and three null results in Eastern which reduces the clarity of the separation with the passage of time.

This is a disagreeable table to make. But if one wishes to understand what is happening it is an essential piece of evidence.

The above results will be discussed later in the context of Pathological Science.

2. BACKGROUND INFORMATION

2.1 Social Responsibility of Scientists.

Almost every newspaper, magazine and television has had several news items, editorials and articles on the subject of Cold Fusion. Many seem to be believe that there are two types of scientists - those in favour of ecologically sound cold fusion and those who cannot find the effect. Most have difficulty in evaluating the evidence and even bigger difficulty in putting it in a historical context. While it will doubtless have some positive effects, it clearly has harmful effects on Science. Thus one man I talked to said that he used to believe doctors never made mistakes until he learnt otherwise, now he feels the same about scientists. Another man I met on the the Mont Fort glacier, kept saying to me "but suppose it is true?" and no matter how I tried to explain about careful experiments with controls and those with inadequate controls, he continued, reasoning that if there was the slightest chance, one should continue. The extreme example of this attitude is yesterday's Figaro Magazine which is widely read, and which has a major and colourful article "Cold Fusion: and if...it were true?". It then examines what would happen starting with a beautiful colour photo-montage of "a veritable scoop" of a photo taken in 73 years time of a cold-fusion power station sticking out of the water, "the heavy water in sea water would be a true richness , a fuel which does not pollute and is available to all" - and there are seven pages of this! Clearly not only Fleischmann and Pons and the University of Utah public relations section, but all scientists have a responsibility to consider these questions.

As these notes are intended for scientists, up to now I have refused to give them to the media, but in view of the problems, this may have been a mistaken decision.

It suddenly struck me last week that something was missing. There were no statements on cold fusion from internationally respected scientists. In the past one would have expected something from a Lord Rutherford, Einstein, Fermi, Blackett, Alvarez, Feynman. But there is a great silence.

2.2 Historical development of Cold Fusion

2.2.1 A Press Conference was called by the University of Utah on 23 March breaking the agreement signed with Brigham Young University. Calling a press conference is not the conventional way to present a new result and while it circumvents the normal process of peer review, is not necessarily bad. There are precedents, e.g. Paul Chu announced high temperature superconductors and when Supernova 1987A was discovered, the IAU was informed immediately and distributed the information quickly. In these two cases, the effect was immediately observed by everyone. Thus if the result is important enough, it is justiable to resort to a press conference - BUT first you must be reasonably convinced your are right and secondly, have made all reasonable checks. In the case of cold fusion, Fleischmann and Pons were convinced, but as Nature said, it is astonishing that they did not carry out the simplest of tests, particularly since they claim to have been thinking about it for over 5 years.

2.2.2 Financial and Legal Considerations

Both the State of Utah and the University of Utah are said to have economic problems. Within days of the press conference, the Utah legislature voted to make available $5 million for cold fusion research. And Congress was asked for money to set up a research centre in Utah. This was strongly supported in Utah and by the Utah press who gave great prominance to any confirmation of Fleischmann and Pons's results. At the Congressional hearings, the president of Utah Univ. asked for $25 million to develop the new discovery. Robert L. Park, head of the APS Washington Office, in his newsletter, says that Gerald Cassidy, the Washington lobbyist who invented "pork barrel science", was hired by the University just two weeks earlier. At the hearings the first five speakers all strongly supported the project; the sixth was Steve Jones of Brigham Young University, also in the State of Utah, who reported his observations of neutrons and said that these were so much lower than what would be expected from Fleischmann and Pons's estimates of fusion giving heat, that they were impossible. Steve told me that he was then attacked in the Utah papers as being disloyal to Utah! Later Dr. Lewis of Cal Tech described his null results and detective work, but according to Park, "Rep. Robert Walker(R-Pa), reportedly annoyed at what he took to be arrogance, refused to withdraw his ammendment to move $5 million from magnetic confinement to cold fusion". However Washington enthusiasm seemed to have cooled as the expected visit of 30 Senators and Congressmen to Utah seems to have failed since there were no takers of the offer by Sen. Jake Garn (R-Utah) and Rep. Wayne Owens(D-Utah).

There are clearly major financial considerations and the U of U are said to have taken out at least 4 patents. A major player is Dr. J. Brophy who is the Vice-President for Research. In the 13 May edition of the Salt Lake Tribune Dr. Brophy said that "some 50 companies.... have signed 'confidential disclosure agreements' allowing them to examine the universities patent applications to see if they want to persue joint development with the school. 'Its not something the companies enter into lightly'". He was asked how the companies might benefit from this, "He cautioned that it is still too early to predict the experiments economic value. The longest the device has been documented to produce energy is about 800 hours, he said, and it hasn't got much hotter than what it takes to boil water, which may not make it a practical energy source 'unless you're just interested in making coffee'". [note, of the results that impressed the audience at CERN when Martin Fleischmann gave his lecture, was his description of how they had left a cell overnight and in the morning they found it had melted and the fume cupboard was smouldering. However this apparently happened in 1985 and there is a persistant rumour (which may not be true) that the son of one of the experimenters was asked in the previous evening to go and turn the current down and it is not clear which way he turned the current.]

The price of palladium has varied appreciably recently. After March 23 it rose sharply from $145 to $180 per ounce but has since fallen to $150, also the number of contracts treated jumped spectacularly. However it is interesting to constate that as the Journal de Geneve observes, the price of palladium has increased steadily over the previous five months from $120 although the other five metals of the platinum group decreased! Now it could be that is normal as it will be used more for car catalysers, but this is still only 6% of its use (it is 51% for electronics and 31% for dentistry).

If the source of the excess heat reported is fusion, then the only possibility left is that helium is produced. It should then be trapped in the palladium electrode in large quantities. At the ECS meeting in Los Angelos several groups (Sandia, Livermore, MIT) offered to analyse small pieces of the electrodes of the labs that were finding excess heat. The replies were; from Pons - previous committments prevent us from releasing our electrodes we made an agreement not to do this - and from Fleischmann - We are not free agents to answer certain questions. ThesE answers, so different from normal scientific exchanges, surprised people. However the May 13 Salt Lake Tribune may help;

"A representative of Johnson Matthey, the supplier of the palladium rods that Stanley Pons and Martin Fleischmann used for their nuclear fusion experiment' has taken some of the rods back to analyse them for the presence of helium or other fusion byproducts.

" University of Utah Vice President James brophy said Friday that the representative came to Utah last week and picked up the rods. Johnson Matthey has facilities all over the world, and Dr. Brophy was not sure where the rods had gone or how long it would be until the results are known.

"Dr. Brophy said Dr. Pons and Dr. Fleischmann had signed an agreement with Johnson Matthey a few months ago 'after they had started getting positive results' under which the company would provide the rods basically for nothing on the condition that they are returned to the company for analysis.

" they agreed to share information about the cathode...,' he said "It's almost like the agreement with Los Alamos"

"Johnson Matthey will not receive a stake in any profits the experiment might generate..."

"I can't imagine any reasonable person turning them over to a competitor like MIT", Dr. Brophy said. "It would drive our lawyers up the wall".

Such paranoia was already evident with the U of U making agreements with Brigham Young University and then breaking them. Similarly the U of U President Chase Peterson seems also caught up. According to the U student newspaper, the Daily Utah Chronicle, they "are not very far ahead of the rest of the world' They are ahead of the world by days, not by years." Later he acknowledged that some researchers are even days ahead of the U. After talking of the Japanese working hard, he said "The pure science surrounding the fusion technique has 'collided with an aglutinated social phenomenon of science and patents, and finance and world geopolitics'. "The pure processes of science are "incompatible with rapid, effective, successful state and national economic development".

It is interesting to compare the atmosphere at the U of U with the "Important Notice" which NSF Director Erich Bloch sent to heads of all instituitions receiving NSF grants. According to Robert Park, it is to implement the Rhodes Report. It calls for significant findings to be submitted for publication promptly and for primary data, samples, etc. to be shared readily with other researchers. Institutional policies should "preserve the prime function of academic institutions as creators and transmitters of knowledge, while safeguarding the independence of the faculty and the interests of the students".

Fleischmann and Pons have said that they will collaborate with Los Alamos. But on 11 May the Salt Lake Tribune wrote that legal problems were holding things up. "We were ready yesterday" said Los Alamos Spokesman, Jeff Schwartz, adding only "talk to Dr. Brophy". "We're ready too" said Dr. Brophy, but the attorneys"are concerned about protecting the university's intellectual property rights while working with a national lab". The Salt Lake law firm retained by the U of U did not return Tribune calls. Later it was said the agreement would be signed the 15 May, but since then it seems to have been put off another week.

2.2.3 Preparations for the Electrochemical Society Meeting

After the APS meeting which Fleischmann and Pons refused finally to attend as they were too busy in Washington (which is not too far from Baltimore), it was expected the ECS meeting would provide a good forum for discussion. However the anouncement read that after the featured speakers, Prof. Fleischmann Pons and Jones, "research groups who have verified the initial reports of Professors Fleischmann and Pons or Professor Jones, are invited to present short (approx. 10 min.) summaries of their work. To make arrangements for a brief presentation of confirmation results, please contact Elton Cairns, LBL". Robert Park could not believe it so called the organiser who explained 'with perfect logic' "since the subject of the session is fusion, papers that don't report seeing fusion would not be relevent".

The Deseret News of 7 May had an article entitled "U scientists await their day in court". "We are going to present new results that exhibit large amounts of excess heat" Pons told. In short the chemists will give a thorough, clean analysis of the thermal part of their experiment. "We can't make people believe it, but we can tell people how we are analysising the data. We can tell them what our measurements are". We will give electrochemists the recipe for their energy source. "We are going to supply all the possible information that we can". "People evidently are misunderstanding a lot about calorimetry. A lot of people are making calorimetric measurements that may not be suitable for these experiments". Pons has repeatedly cautioned that some labs have been in too big a hurry to get confirmations. "We have had abundant criticism ..... primarily from physicists who have been having a good laugh in recent weeks at the expence of the researchers, the U and the State of Utah" "Fantastic, no problem. Criticism does not bother me. What bothers me is that these people are not doing the science. They are philosophizing, using a physical theory, parts of which may be totally wrong. Prey, where is their data? We are presenting ours. Why don't they publish their experimental procedures so that we can see what they are doing to disprove our work. It is very easy to pontificate and to say someone's data is wrong; proving it is another matter". Pons said he and Fleischmann haven't seen anyone disprove their data. "If they do, we will certainly admit it; if they don't, they will have to stand corrected".

Dr. Brophy said that it was "awfully difficult for me to believe that experimental electrochemists who have been working on their experiment for 5.5 years" could have their results dismissed by researchers who had worked on the problem for only five weeks.

It may be noted that most of these comments that differ from those of people outside Utah, come from Prof. Pons and Dr. Brophy but not from Martin Fleischmann.

3. COMPARISON WITH CHARACTERISTICS OF PATHOLOGICAL SCIENCE

The father of Pathological Science or the study of Wrong Results in Science, was the great physical chemist Irving Langmuir. I have been studying it since the splitting of the A2 that I was close to, some 20 years ago, and since 13 years have been giving occasional lectures on the subject after further work. Originally Irving Langmuir gave 6 characteristics by which one could recognise wrong results. However when I took a somewhat larger sample of cases and made a two dimensional array, I found that only 2 to 5 of the characteristics were fulfilled which was too weak statistically. Also I could identify 6 other characteristics. Then each case fulfilled about 6 to 8 of the 12 characteristics which was more satisfactory.

After hearing Martin Fleischmann's inspirational talk at CERN on 30 March, I found that his work only fulfilled two of the characteristics, namely

5. "Unusual or rare materials used"
12. "Unusual or surprising result".

Since then times have changed and more characteristics are satisfied;

7. "Understanding experiment may depend on not available details". Because of this I asked Steve Jones, if he had any special secret - he said "no" and laughed. Similarly on March 30, Martin Fleischmann gave the impression it was all simple and straight forward. To be sure, I asked him a few days later if there was any special way in which the rod had to be treated - he said no, it was a volume effect and therefore the surface treatment did not matter. However as has been shown above, Prof Pons suggests that the rod must be cast and not annealled, and Prof Huggins says the hydrogen must be driven out first.

6. "Great reluctance to change the experimental conditions". Although Fleischmann and Pons say they have been working on the subject for 5 1/2 years, it is amazing how few experiments they have tried whereas others have varied conditions and materials and salts and looked for dynamic rather than stable effects.

4. "Statistics small, special - no point in doing a statistical analysis". One would have expected hundreds of tests to have been made and statistics to have been presented, but the best we have is a recent statement that now 90% of the tests are successful - but the actual numbers are not given though it is hard to see why a patent lawyer would forbid it.

10. "Always a quick and ready answer to objections - though often hard to check answer". A perfect example is the 8mm diameter rod that was declared to be "dead", but the reasons given are hard to tie down or to test.

11. "The experiment develops in three phases;

Phase 1. the original results are presented and quickly confirmed by several other experimental groups.

Phase 2. gradually a few null results are presented and then there is a period of confusion where there are equal numbers of confirmations and null results.

Phase 3. All the results are null".

W4e have gone through Phase 1 and are in Phase 2. However this needs some explanation. If a group tries to confirm, it tends to do it quickly and in its rush, if it finds the effect, feels that it has acheived a good and correct result and therefore it is safe to publish (or in the modern way, to call a press conference). If however the group does a careful job making full checks and controls, and finds no effect, it is naturally reluctant to publish because;

a) it may have made a mistake since it disagrees with published results so that the penalty for error is greater

b) there may be some secret that only the original authors know.

So the group with a null result is slower to publish.

Once it becomes widely known that some people have found a null result, then all the other groups that have found null results and are holding back, now decide to publish. And the situation is reversed - the groups that found a positive confirmation now feel they should check further, and when they do proper checks, also find null results.

In this special case of Cold Fusion, there is a new factor. Communications are fairly fast nowadays, so that the big news story that Cold Fusion had been found, went round the world very quickly and everywhere groups tried to repeat the effect. In two regions of the world, North-Western Europe and USA Region 1, there is an excellent network of instantaneous communication so that people quickly realised that their neighbours also had trouble repeating the experiments. Since they had lots of excellent equipment already available they could do experiments quickly. So in these two regions 20 out of 21 groups (exception was Stanford), found and reported null results. In the other regions the teams had often less equipment available and their communications were less good so that out of perhaps a hundred groups, only those few with apparent confirmation, presented their results. The numbers of null results is increasing with time but at present it stands at (29-1) confirmations and 5 null results. The difference between the parts of the world is statistically significant.

There is as far as I can observe, a correlation between the media in the two parts of the world. In NW Europe and the greater NYT region, a substantial fraction of the media do not simply believe every press release, but check to see if outsiders consider it reasonable and if they agree. This healthy dose of scepticism is a great saving. So the result is that NW Europe and the USA Region 1 are in Phase 3 while other parts of the world are in Phases 1 and 2.

Thus 7 out of the 12 characteristics of a Wrong Result appear to be fulfilled and this is the normal proportion.

Characteristic number 9 - "Great confidence -You must believe" is almost fulfilled from what we have heard of the atmosphere in Utah (for which I thank correspondants there).

Generally in talking about Wrong Results in Science, I only treat cases of more than forty years ago to avoid personal feelings, but in view of the harm being done, this is a special case.

It is important to emphasise that there is no hint that anyone was deliberately cheating or falsifying data. In Pathological Science as in the cases Randi has dealt with, 95% of the people concerned are deceiving themselves - their desire and belief in the result is so strong that their judgement weakens and they ignore and do not seek for any negative evidence. The indications of paranoia shown in the quotations above, illustrate this. This week's quotation in CERN chosen by R. Carreras is from Nietzsche and says that "Convictions are more dangerous foes of truth than lies"

4. CONCLUSIONS

I hope there will be a wide discussion of all aspects of the subject by all parties at the Santa Fe Workshop and that the whole subject will quickly fade away after that though there will always be some discussion. In Pathological cases, the people who first produced the wrong result, generally never admit they made a mistake, except in a few cases like Paneth who was a great scientist. Thus Blondlot, the discoverer of N-Rays wrote a book afterwards, but everyone had already stopped discussing the subject.

After the turmoil of the last few weeks, it would be good to remember that we all make mistakes and to be humble and sympathetic.

Douglas R. O. Morrison

PS Have just received from Petros Rapidis the latest bulletin from Robert Park who says that the Santa Fe workshop will have a near capacity crowd of 1500. Although the preliminary programme gave top billing to Fleischmann and Pons, they seem to have decided to cancel their appearence (University lawyers apparently worried that Pons might reveal the secret formula) - hope this is not correct and that they do turn up to discuss in the normal scientific manner. He says the Santa Fe non-confrontation will be broadcast live by satellite.

Also Park reports that the Cold Fusion Panel of the Energy Research Advisory board will be co-chaired by physicist Norman Ramsey and nuclear chemist John Huizenga. The 20-man panel should give a preliminary report by Aug 31 and a final report by 15 Nov.

Dear E632 and WA84 Colleagues, 18 May 1989.

COLD FUSION NEWS No. 13

by D.R.O. Morrison, CERN

Introduction

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INTRODUCTION

After the American Physical Society meeting in Baltimore, 1 and 2 May, it seemed to most present that the four original experiments of Fleischmann and Pons were mistaken. However since then there has been the meeting of the Electrochemical Society in Los Angeles on Monday 8 May where despite an absence of details, they had some media success and some new confirmations. Also the results that I have received tend to be more confirmations than null results, in particular there is a preprint from the Bologna - Brigham Young Collaboration in the Gran Sasso tunnel which confirms the result of Jones et al. These new results will be reported and discussed. Thus the question may not be settled until the Workshop on Cold Fusion to be held at Santa Fe from 23 to 25 May. It is sponsored by Los Alamos and DOE - and since Fleischmann and Pons have announced that they are collaborating with Los Alamos only, they can hardly avoid turning up without losing all scientific credibility. This could be a decisive meeting.

This note is in two distinct parts;

1. A presentation of the situation and the new results. Evidence is given for the regionalistion of results. This part will be essentially conventional science.
2. However it is felt that Cold Fusion cannot be understood purely from the results provided by the different scientific groups, as is shown by the evidence for the regionalistion of results which is contrary to our basic belief that the Laws of Science are universal and should be the same in all parts of the world. Hence the second part gives some background that may help to explain the observed regionalisation of results and the disagreements between the results. The subject of Cold Fusion is discussed in terms of Pathological Science and some historical perspective is given.

Here we will again emphasis that there are two main experiments in the so-called Cold Fusion subject, and these should be treated separately. First there is Calorimetry and secondly observation of fusion products.

A) Calorimetry

Claims have been made of unusual heat production, and Fleischmann and Pons say this can be about 10 Watts per cm3 of Palladium. The origin(chemistry, fusion, ...) of this power is not established though people would expect it to be fusion. It is this experiment by Fleischmann and Pons which raised world-wide the hope of unlimited power from water with little attendant pollution.

B) Fusion Products

Claims of observation of neutrons, gammas, tritium and helium as evidence of fusion have been made, but the corresponding power is very much less, about 1 E-9 to 1 E-13 watts which would be much less than the power put in and completely useless for power production.

The important point is that up till now none of the observations claimed of fusion products has supported the hope from the calorimetric results that economically useful power can be produced.

1. NEW RESULTS, THEORY AND REGIONALISION OF RESULTS

1.A. CALORIMETRY

At the Los Angeles meeting of the Electrochemical Society which was attended by some 2000 people, there was a special session on Cold Fusion. What follows is from many different sources (unfortunately none of them scientific papers!), so is liable to error.

Fleischmann and Pons had said they would produce new results but there was some disappointment as they not give any more details of their experiment or new results except to claim that they now had much more power, about 10 to 50 times more than they had put in (previously was 4 to 8 times input). But no detailed justification was presented. According to AP they observed this energy as a burst of heat that lasted two days. Previously Pons had been reported as saying that when they tried normal water instead of D2O, they had obtained excess heat also. However the report by Nature quotes Fleischmann as saying 'any suggestions that control experiments done with ordinary water were yielding excess heat comparable to that produced with heavy water were "total nonsense"'.

Possibly the best summary of what they said is in the Utah paper, the Deseret News;

"Pons and Fleischmann reported;

-- experiments continue to show large releases of heat
-- total watts put into the cell are less than the total energy output (without considering the electrochemical reaction)
-- energy coming out of the system is fairly constant, but in some situations there are large bursts of energy
-- The bursts are enormous and, if persistant, are capable of literally boiling the cell out at a very low voltage
-- one cell, running at 32 degrees for 5 1/2 million seconds {about two months} suddenly burst up to 60 degrees and remained at that temperature for several hours
-- bursts of neutrons and other radioactive particles, usually ignored by physicists, have been seen
-- some bursts have lasted long enough to enable scientists to go into the machine to check instruments
-- the heat output from the sustained bursts over a two day period have been between 1000 and 5000 percent more than the input."

[comment - there seems to be more information here than in any other report that I have read on the Electrochemical meeting. Also this is the first time that I have seen a value of temperature quoted (there is none in their published paper)].

At this meeting they were supported by Huggins et al. of Stanford who said that he had observed excess heat with coin-shaped palladium rods which were totally submerged. He assumed that there was no recombination of gases. When H2O was added to reduce the concentration of D2O, the heat generation stopped. He said that the cell with D2O ran consistently hotter than a H2O one.

Dr. Landau of Case Western Reserve claimed to have observed 40% more excess heat than Fleischmann and Pons had reported with D2O. He said there was no heat generated above error with H2O. He reported no recombination of D2 and O2 to 3% accuracy.

Two groups from Texas A and M, Appleby et al. and Bockris et al., reported excess heat. {Earlier it was written in New Technology Week, that the University had devoted 30 of their 70 electrochemists to replicating F&P's experiments. They used a 0.1 cm diameter rod which was 5 cm long. Packham said they "detected a 60 to 70% heat excess" compared to the current input to the electrode of 60 - 100 mA per cm2. The experiment produced a 3 to 4 degree difference in temperature with 4 watts of electrical input}.

[Comment - up to now the only quantitative estimate of excess heat expressed as watts per cm3 of Pd comes from Fleischmann and Pons - it would be good if the other groups would give values in watts per cm3 also].

A major criticism of the Cal Tech group of Lewis et al. and Meyerhof et al., was that there was no stirring in the electrolyte of the cells so that temperature gradients were present and this would mean that the temperature read would vary according to the position of the thermometer. Then since Newtons Law of cooling was used to establish the energy produced, this would affect the calculation of excess heat. This was answered by Fleischmann who showed a video of a cell bubbling energetically and when a dye was added to the liquid, it mixed in about 20 seconds. This was presented as evidence that there were no temperature gradients [comment - it would have been more decisive if the thermometer had been moved around or an array of thermometers had been used].

An even stronger argument against the effect of stirring comes from the Stanford Daily of Wed. 10 May, where Prof. Huggins is quoted as saying that "he has performed experiments in which he stirred the water at different rates demonstrating that the effect continues"

There was some discussion of the reproducibility of the results. All agreed it was not 100%. In their original paper, Fleischmann and Pons mentioned 5 cathodes of which 4 gave excess heat and the fifth was dead "because it gave no excess heat". Huggins suggested it had to be cast and not annealed and should be purged of hydrogen first, but Landau of Case Western Reserve Univ. said they had used annealed cathodes. Bockris said that only 5 or 6 out of 20 palladium electrodes had given excess heat. Lewis said they had used 50 different electrodes, some cast, some annealed and had observed no effect. Earlier in the Deseret News Pons was quoted as saying that from failures round the country, he had deduced the connection between cast Pd and success and milled Pd and failure. Originally they had often failed to generate heat, but now they can get working cells "90% of the time".

It was generally considered that if the excess heat were due to fusion but that the process was such that conmventional fusion products were not observed in sufficient quantities to explain the effect, then the only solution would be that helium was formed with energy and this energy was not emitted as gammas but converted into heat. This would mean that the palladium rods must contain a large amount of helium. Earlier Fleischmann and Pons had said that they had observed helium (Nature 338 (1989) 691) but at Los Angeles they said that this result was premature [other reports indicate that the problem concerns the amounts of background helium, as plagued the 1926 experiments of Paneth and Peters].

A group at the University of Florida, Gainsville, L. Muga et al. report that after several weeks running, they do not observe any abnormal heat effects.

A group at Harwell led by David Williams, is doing a large number of experiments and will not publish for another month. When interviewed by the Sunday Times, David said that no excess heat was observed. What is of special interest is that they are doing very careful experiments, in particular their calorimetric experiments will be at a moderately high constant temperature and then varying the amount of heat input to balance any heat excess or or deficit, which is a much safer technique than observing temperature rises and than using Newton's Law of cooling to deduce the amount of energy produced.

A paper has been distributed electronically by the Tata Institute for Fundamental Research, Bombay, by E. Krishnakumar, et al. entitled; "Calorimetric Studies of Electrolysis of D2O and H2O using a Palladium Cathode" It is unusual in this field in that it gives a detailed description of what was done. Two cells were run for a total of 200 hours, one with D2O and the other with H2O. They were run at four successive current densities, firstly at 25 and 24 mA cm-2 for D2O and H2O resp., the difference being to keep the power levels equal at 0.06 watts. Secondly at 50 and 52 mA giving powers of 0.170 and 0.172 W resp. Thirdly with 100 and 110 mA giving 0.43 and 0.42 W resp. and finally with 200 and 210 mA resp. (power not given). They observe that the D2O cell has a higher temperature than the H2O cell for phases 2, 3, and 4. being about 2 centigrade degrees difference. Care had to be taken to maintain the levels of the electrolyte constant as when this was not done, differences were found. They say that they did not find dramatic effects as others had done (one reference to this is K.S.V. Santhaman et al. Indian J. of Tech. 27 (1989) 175). The paper is said to contain what I have long asked for, that is graphs of the variation of temperature with time - but unfortunately the electronic transmission means that some words are lost and more importantly, the interesting graphs!

[Comments.
(1) The major comment of Lewis et al. was not answered. This was the taking the published numbers of Fleischmann and Pons and making assumptions based on detective work, to calculate the power in and the power out. It was found that the cell is acting as a refrigerator which is not cooling down as much as expected. This lack of cooling down is treated by F&P as excess heat produced and who then by a series of assumptions and extrapolations derive a large power output of many Watts. So far I have not seen any denial of the numbers used by Lewis or refutation of his method - but maybe these were made but not repeated in Nature or the press or people present.
(2) The "confirmations" of excess heat of Stanford, Texas A&M (twice) and of Case Western Reserve seem to derive from an observation that the Pd electrode in the D2O ran at a higher temperature than the one in H2O as is described in detail by the Tata Institute group. If one assumes this difference is a proof of excess heat, then one could calculate this as Watts per gm of Palldium. However it does not seem to answer the argument of Lewis et al. To answer it one needs a full account of all the energy flows, assumptions, voltages and the actual calculation. So far I have not heard of such a presentation. Better still would be a good calorimetric experiment with a closed system and the apparatus kept at constant temperature.]

1.B. FUSION PRODUCTS

At the Electrochemical conference, Prof. Fleischmann said that they realised that the gamma ray line that they claimed to have observed at 2.2 MeV was too narrow and too weak to be from neutron capture. [this was generally taken as a retraction and follows criticism that their counter did not have enough resolution to give such a narrow peak - this is apart from worries about background from natural sources such as radon or thorium decays products.

There have been several reports of the observation of fusion products by Southern European groups, but in recent conversations, the repetition of some experiments are meeting problems of reproducibility.
However there is an important new preprint from an experiment in the Gran Sasso tunnel where the background from Cosmic Rays is expected to be very low as they are under 4000 metres of water equivalent. This experiment is being performed by the Bertin et al. group of the Univ. of Bologna and the INFN, Bologna together with the Brigham Young group of Jones et al. Typically three cells were operated simultaneously with cathodes of fused titanium pellets of about 1 gram. The DC power supply was 6-10 Volts and the currents 40 to 60 mA. The 2 neutron counters were with NE213 scintillator liquid and n-gamma discrimination was obtained by gating fast and slow pulses. The cells were kept in front of counter A while the other counter B was at 8 metres. "After about one hour following the connection of the cells to the power supply, the counting rate defined this way, markedly increased, went through a maximum and eventually dropped (in about three hours) at the level of the background counts." This was observed twice more. The raw data are not shown, but the background-subtracted data are given and show a neutron energy distribution which agrees very well with the Monte Carlo expectation for 2.45 MeV neutrons and corresponds to 350 +/- 51 neutrons. It looks impressive, but some

comments can be made;

1. It is written more than once that the results are preliminary. Now much of the trouble with the subject of cold fusion is groups publishing or presenting preliminary results - at this stage it is not necessary. We need solid results.
2. The total cell operation period was 10 hours. This is very short for an important result. A few days more would have allowed time for basic controls that have not been done.
3. Fleischmann and Pons were often criticised for placing their calibration counter at a distance - here counter B is placed at 8 metres. It has been repeatedly said that the correct way is to intersperse controls e.g. to run with H2O, and also to run with a platinium cathode instead of a titanium one. In view of the relatively high statistics claimed, it should be possible to do all of these in a few days.

Another experiment has been done deep underground. This is by the Bugey - Grenoble Collaboration that had previously found no neutrons using an exceptionally good detector. They measured neutrons again but with a less sophisticated detector, but still a good one, in the Frejus tunnel. Yves Declais told me that their cosmic ray induced background is about one per year, but due to natural radioactivity, their actual background is 0.05 neutrons/hour or about one a day. They tried a number of different ways of inducing cold fusion including electrolysis and the techniques used by Prof. Scaramuzzi of Frascati of using pressure to look for non-equilibrium effects. They found no increase in the number of neutrons recorded. Thus this experiment is in contradiction with the results of Frascati, Gran Sasso and the original results of Jones et al. Yves will present these results at the Los Alamos meeting next week.

There was a report by press conference, from the University of Florida, Gainsville, of the observation of tritium production from a cell. This was interpreted by the authors, two emeritus professors of nuclear engineering, as confirming Cold Fusion. However another group at Gainsville, L. Muga et al., comprising nuclear chemists, condensed matter physicists and nuclear physicists, do not find any tritium production after running for several weeks - they checked carefully for tritium contamination. Also Muga et al. do not observe neutrons or gammas [unfortunately I do not have numerical limits].

A paper "Deuterium Nuclear Fusion in Metals at Room Temperature" by P. Thomas et al. of the "Ruder Boskovic' Institute, Zagreb, Yugoslavia, reports on neutron measurements from a three-compartmental electrolytic cell. The Palladium cathode was surrounded by a platinium wire anode but it was found after a time that the anode had dissolved. Using X-ray fluorescence spectroscopy it was found that there was a thin platinium layer on the palladium cathode. As this layer might hinder the passage of deuterium into the palladium, a large palladium sheet was then used as the cathode. A 6Li-glass scintillator was used as a neutron detector. Using a 252 Cf source its efficiency was found to be 1%. The total counting rate over all channels was 0.15 impulses/sec. The counting rate for neutrons was 0.01 per sec. The experiment lasted 3 E5 seconds. The amount of deuterium in the palladium was checked by weighing. Typical values of 0.15 grams of deuterium per 14 grams of palladium were found. No significant deviation was observed, this gave an upper limit of 0.01 neutrons per gram of palladium. In preliminary experiments using titanium powder under pressure and cooled to liquid nitrogen temperatures, it was found that the cooling affected the stability of the neutron detector [and no results were given].

Prof. Leonid Ponomarev has informed me of several experiments being performed at the I. V. Kuratchov Institute of Atomic Energy using Pd and Pt electrodes and D2O and LiOD electrolyte. The sorption and desorbtion of deuterium was studied at different temperatures and pressures(up to 1.5 Kg). The counters for neutrons were scintillators, BF3, and 3He while for gammas a NaI counter was used. No gammas were found. For neutrons the result was (1 +/- 1) E-3 neutrons per sec per gram of Pd which is much less than reported by Fleischmann and Pons and less than Jones et al.

[Have been told of several other experiments all of whom have null results but for various reasons, do not wish to publish - one experimenter summed it up as "no neutrons, no fusion, and no publication"].

1.C. THEORY

The famous chemist Linus Pauling has had a letter published in Nature [as he is 88 years old, is this a record?]. He recalls a paper in Physical Review that he wrote in 1938 where he pointed out that in transition metals, 0.72 orbital per atom of the orbitals in the outer shell remains apparently unused. He identified the 0.72 orbital per atom as the metallic orbital which gives metallic properties. He noted that this could explain why palladium is saturated with hydrogen at the composition PdH(0.6). He judges that in F&P's experiment, the deuterium is being forced into forming an unstable higher deuteride such as PdH(2). "The subsequent decomposition of which would give an increased evolution of heat or to the explosion of the deuteride into palladium powder and hydrogen gas".

The language that Pauling uses is different from those of papers I have read recently and is a useful complement giving a return to some fundamentals. It is considered that the octahedral sites are occupied first then more tetrahedral ones. For PdH(x) where x is about 0.6 to 0.7, there are some phase changes in the system, the exact value depending on temperature. These effects can be studied by neutron scattering or by implanting polarised muons. Keith Ross of Birmingham has given me the following references;

"Implanted muon study of supperlattice ordering in palladium hydride PDH(0.64)" by S.F.J. Cox et al. Hyperfine Interactions 31 (1986) 105 - 111

"Interatomic potentials and lattice distortions in PdD(0.8)" by M.W. McKergow et al. Zeits. fur Phys. Chemie neue folge Bd. 146.S (1985) 159 - 169.

Incidently he also gave me an interesting paper on "A proposed method of hydrogen isotope separation using palladium alloy membranes" by J. Evans et al. Journal of Less Common Metals, 89 (1983) 407 - 414. The idea would be to separate hydrogen, deuterium and tritium in a fusion reactor (he means a hot fusion reactor!) This work may have some relevence to reports on the identification of tritium from cells.

[A point I would like to make again is that there is a tremendous literature on palladium hydrides and deuterides and it is surprising that people can think of the subject for years and not go to libraries.] Prof. Ponomarov send me an extract from the famous paper of S.S. Gerstein and Ya Zeldovitch, Sov. J. Uspekchi 61 (1960) where they talked of possible fusion 39 years ago;

"At first glance it seems that the necessary reduction in distance between the hydrogen nuclei may occur in certain chemical compounds; actually however in all known compounds containing hydrogen the distance between the hydrogen nuclei is greater than in the H2 molecule. Thus, interatomic separations in ordinary molecules are too large for the yield of sub-barrier nuclear reactions between hydrogen isotopes to have any significant magnitude".

In support of this, let me repeat the appropriate separations for Palladium;

palladium-palladium in metal	3.89 A
palladium-palladium in PdH(x)	4.03 A
deuterium-deuterium in octahedral sites	2.85 A
deuterium-deuterium in octahedral -tetrahedral sites	1.74 A
deuterium-deuterium in free gas molecule	0.74 A

(information from Prof. Pui Lam of Hawaii).

Nature also published a letter from J Gittus (UKAEA) and J. Bockris (Texas A&M) who presented a positive result on calorimetry at Los Angeles. They say that it is well known that hydrogen tends to segregate in cracks, grain boundaries or interfaces, and dislocations. They suggest "prior accumulation of hydrogen in Pd may prevent the build up of deuterium and this could explain why cold fusion is so hard to replicate". "When the concentration (of deuterium) in these local regions has reached a high enough level, the reaction commences in the segregated region. We are unable to say just what the reaction is, but it may be a form of nuclear fusion or some chemical reaction." "If this hypothesis is correct, it accounts for the failure of some laboratories to produce the 'fusion' reactions described by Fleischmann and Pons". [comment, both the Yale - BNL and Cal Tech experiments purified their Pd electrodes before starting, so their observation of no fusion cannot be described as a "failure"].

Another interesting hypothesis is "Scenario for energy production from cold fusion by free quark catalysis" by G.L.Shaw et al. They "present a scenario in which small numbers of free stable ubar-ubar = Q anti-diquarks (electric charge of -4/3, mass of a few GeV, and short range strong repulsion with hadrons) catalyse the deuteron fusion. The reaction channel He4 + Q dominates". They say 100 Q's are enough to give watts of power and "although no quark search has been made using Pd, Fairbank and collaborators found evidence for free quarks on little spheres of the transition metal niobium at a level as large as 1 quark per 1 E19 Nb molecules". However they predict neutrons at much higher rates than have been observed.

1.D REGIONALISATION OF RESULTS

At the APS meeting in Baltimore, I presented tables of results to justify the conclusion that confirmation and non-confirmation of the five positive results (calorimetry, neutrons, gammas, tritium, and helium) claimed by Fleischmann and Pons, were not uniformly distributed throughout the world, but rather confirmations occurred in certain regions and null findings in other regions. A full update will be given at the Los Alamos Workshop, and here will give a summary.

	CONFIRMATION		CALORIMETRY		FUSION PRODUCTS	
	Yes	No	Yes	Null	Yes	Null
NORTH-WEST EUROPE	0	11	0	3	0	8
USA REGION 1	1	10	1	1	0	14
USA REGION 2	10 - 1	1	6	0	7 - 2	0
SOUTHERN EUROPE	5	0	0	0	5	0
EASTERN EUROPE	6	2	2	0	5	3
REST OF WORLD	8	2	3 + ?	2	4	4

Some comments are required;

1) The information is not always easy to obtain or evaluate, therefore is liable to some error, but not enough to alter the conclusions. For example I have heard that in Southern Europe, there are 9 groups that find positive effects, not 5, but I have inadequate information on four of them.

2) The negative signs in the table are retractions (10 - 1, the one is Georgia Tech),(7 - 2, the two are Fleischmann's comments on their gamma and helium measurements).

3) The ? is for the Tata Institute's finding that the cathode runs hotter with D2O than with H2O - they do not draw any conclusions about this being a heat excess.

4) The United States is devided into two regions;

USA REGION 1 comprises that part of the country that is well informed by the New York Times style of science writing that thinks and asks before it publishes, plus the major research centres. The one positive finding is Prof Huggins at Stanford. I know of several other non-confirmations but the authors do not wish to publish or announce it.

USA REGION 2 comprises the rest of the country.

5) Each fusion product result is counted once, e.g. a group that it did not find neutrons nor gammas would be counted as two null results.

6) Since the APS meeting, there have been two null results in The Rest of the World and three null results in Eastern which reduces the clarity of the separation with the passage of time.

This is a disagreeable table to make. But if one wishes to understand what is happening it is an essential piece of evidence.

The above results will be discussed later in the context of Pathological Science.

2. BACKGROUND INFORMATION

2.1 Social Responsibility of Scientists.

Almost every newspaper, magazine and television has had several news items, editorials and articles on the subject of Cold Fusion. Many seem to be believe that there are two types of scientists - those in favour of ecologically sound cold fusion and those who cannot find the effect. Most have difficulty in evaluating the evidence and even bigger difficulty in putting it in a historical context. While it will doubtless have some positive effects, it clearly has harmful effects on Science. Thus one man I talked to said that he used to believe doctors never made mistakes until he learnt otherwise, now he feels the same about scientists. Another man I met on the the Mont Fort glacier, kept saying to me "but suppose it is true?" and no matter how I tried to explain about careful experiments with controls and those with inadequate controls, he continued, reasoning that if there was the slightest chance, one should continue. The extreme example of this attitude is yesterday's Figaro Magazine which is widely read, and which has a major and colourful article "Cold Fusion: and if...it were true?". It then examines what would happen starting with a beautiful colour photo-montage of "a veritable scoop" of a photo taken in 73 years time of a cold-fusion power station sticking out of the water, "the heavy water in sea water would be a true richness , a fuel which does not pollute and is available to all" - and there are seven pages of this! Clearly not only Fleischmann and Pons and the University of Utah public relations section, but all scientists have a responsibility to consider these questions.

As these notes are intended for scientists, up to now I have refused to give them to the media, but in view of the problems, this may have been a mistaken decision.

It suddenly struck me last week that something was missing. There were no statements on cold fusion from internationally respected scientists. In the past one would have expected something from a Lord Rutherford, Einstein, Fermi, Blackett, Alvarez, Feynman. But there is a great silence.

2.2 Historical development of Cold Fusion

2.2.1 A Press Conference was called by the University of Utah on 23 March breaking the agreement signed with Brigham Young University. Calling a press conference is not the conventional way to present a new result and while it circumvents the normal process of peer review, is not necessarily bad. There are precedents, e.g. Paul Chu announced high temperature superconductors and when Supernova 1987A was discovered, the IAU was informed immediately and distributed the information quickly. In these two cases, the effect was immediately observed by everyone. Thus if the result is important enough, it is justiable to resort to a press conference - BUT first you must be reasonably convinced your are right and secondly, have made all reasonable checks. In the case of cold fusion, Fleischmann and Pons were convinced, but as Nature said, it is astonishing that they did not carry out the simplest of tests, particularly since they claim to have been thinking about it for over 5 years.

2.2.2 Financial and Legal Considerations

Both the State of Utah and the University of Utah are said to have economic problems. Within days of the press conference, the Utah legislature voted to make available $5 million for cold fusion research. And Congress was asked for money to set up a research centre in Utah. This was strongly supported in Utah and by the Utah press who gave great prominance to any confirmation of Fleischmann and Pons's results. At the Congressional hearings, the president of Utah Univ. asked for $25 million to develop the new discovery. Robert L. Park, head of the APS Washington Office, in his newsletter, says that Gerald Cassidy, the Washington lobbyist who invented "pork barrel science", was hired by the University just two weeks earlier. At the hearings the first five speakers all strongly supported the project; the sixth was Steve Jones of Brigham Young University, also in the State of Utah, who reported his observations of neutrons and said that these were so much lower than what would be expected from Fleischmann and Pons's estimates of fusion giving heat, that they were impossible. Steve told me that he was then attacked in the Utah papers as being disloyal to Utah! Later Dr. Lewis of Cal Tech described his null results and detective work, but according to Park, "Rep. Robert Walker(R-Pa), reportedly annoyed at what he took to be arrogance, refused to withdraw his ammendment to move $5 million from magnetic confinement to cold fusion". However Washington enthusiasm seemed to have cooled as the expected visit of 30 Senators and Congressmen to Utah seems to have failed since there were no takers of the offer by Sen. Jake Garn (R-Utah) and Rep. Wayne Owens(D-Utah).

There are clearly major financial considerations and the U of U are said to have taken out at least 4 patents. A major player is Dr. J. Brophy who is the Vice-President for Research. In the 13 May edition of the Salt Lake Tribune Dr. Brophy said that "some 50 companies.... have signed 'confidential disclosure agreements' allowing them to examine the universities patent applications to see if they want to persue joint development with the school. 'Its not something the companies enter into lightly'". He was asked how the companies might benefit from this, "He cautioned that it is still too early to predict the experiments economic value. The longest the device has been documented to produce energy is about 800 hours, he said, and it hasn't got much hotter than what it takes to boil water, which may not make it a practical energy source 'unless you're just interested in making coffee'". [note, of the results that impressed the audience at CERN when Martin Fleischmann gave his lecture, was his description of how they had left a cell overnight and in the morning they found it had melted and the fume cupboard was smouldering. However this apparently happened in 1985 and there is a persistant rumour (which may not be true) that the son of one of the experimenters was asked in the previous evening to go and turn the current down and it is not clear which way he turned the current.]

The price of palladium has varied appreciably recently. After March 23 it rose sharply from $145 to $180 per ounce but has since fallen to $150, also the number of contracts treated jumped spectacularly. However it is interesting to constate that as the Journal de Geneve observes, the price of palladium has increased steadily over the previous five months from $120 although the other five metals of the platinum group decreased! Now it could be that is normal as it will be used more for car catalysers, but this is still only 6% of its use (it is 51% for electronics and 31% for dentistry).

If the source of the excess heat reported is fusion, then the only possibility left is that helium is produced. It should then be trapped in the palladium electrode in large quantities. At the ECS meeting in Los Angelos several groups (Sandia, Livermore, MIT) offered to analyse small pieces of the electrodes of the labs that were finding excess heat. The replies were; from Pons - previous committments prevent us from releasing our electrodes we made an agreement not to do this - and from Fleischmann - We are not free agents to answer certain questions. ThesE answers, so different from normal scientific exchanges, surprised people. However the May 13 Salt Lake Tribune may help;

"A representative of Johnson Matthey, the supplier of the palladium rods that Stanley Pons and Martin Fleischmann used for their nuclear fusion experiment' has taken some of the rods back to analyse them for the presence of helium or other fusion byproducts.

" University of Utah Vice President James brophy said Friday that the representative came to Utah last week and picked up the rods. Johnson Matthey has facilities all over the world, and Dr. Brophy was not sure where the rods had gone or how long it would be until the results are known.

"Dr. Brophy said Dr. Pons and Dr. Fleischmann had signed an agreement with Johnson Matthey a few months ago 'after they had started getting positive results' under which the company would provide the rods basically for nothing on the condition that they are returned to the company for analysis.

" they agreed to share information about the cathode...,' he said "It's almost like the agreement with Los Alamos"

"Johnson Matthey will not receive a stake in any profits the experiment might generate..."

"I can't imagine any reasonable person turning them over to a competitor like MIT", Dr. Brophy said. "It would drive our lawyers up the wall".

Such paranoia was already evident with the U of U making agreements with Brigham Young University and then breaking them. Similarly the U of U President Chase Peterson seems also caught up. According to the U student newspaper, the Daily Utah Chronicle, they "are not very far ahead of the rest of the world' They are ahead of the world by days, not by years." Later he acknowledged that some researchers are even days ahead of the U. After talking of the Japanese working hard, he said "The pure science surrounding the fusion technique has 'collided with an aglutinated social phenomenon of science and patents, and finance and world geopolitics'. "The pure processes of science are "incompatible with rapid, effective, successful state and national economic development".

It is interesting to compare the atmosphere at the U of U with the "Important Notice" which NSF Director Erich Bloch sent to heads of all instituitions receiving NSF grants. According to Robert Park, it is to implement the Rhodes Report. It calls for significant findings to be submitted for publication promptly and for primary data, samples, etc. to be shared readily with other researchers. Institutional policies should "preserve the prime function of academic institutions as creators and transmitters of knowledge, while safeguarding the independence of the faculty and the interests of the students".

Fleischmann and Pons have said that they will collaborate with Los Alamos. But on 11 May the Salt Lake Tribune wrote that legal problems were holding things up. "We were ready yesterday" said Los Alamos Spokesman, Jeff Schwartz, adding only "talk to Dr. Brophy". "We're ready too" said Dr. Brophy, but the attorneys"are concerned about protecting the university's intellectual property rights while working with a national lab". The Salt Lake law firm retained by the U of U did not return Tribune calls. Later it was said the agreement would be signed the 15 May, but since then it seems to have been put off another week.

2.2.3 Preparations for the Electrochemical Society Meeting

After the APS meeting which Fleischmann and Pons refused finally to attend as they were too busy in Washington (which is not too far from Baltimore), it was expected the ECS meeting would provide a good forum for discussion. However the anouncement read that after the featured speakers, Prof. Fleischmann Pons and Jones, "research groups who have verified the initial reports of Professors Fleischmann and Pons or Professor Jones, are invited to present short (approx. 10 min.) summaries of their work. To make arrangements for a brief presentation of confirmation results, please contact Elton Cairns, LBL". Robert Park could not believe it so called the organiser who explained 'with perfect logic' "since the subject of the session is fusion, papers that don't report seeing fusion would not be relevent".

The Deseret News of 7 May had an article entitled "U scientists await their day in court". "We are going to present new results that exhibit large amounts of excess heat" Pons told. In short the chemists will give a thorough, clean analysis of the thermal part of their experiment. "We can't make people believe it, but we can tell people how we are analysising the data. We can tell them what our measurements are". We will give electrochemists the recipe for their energy source. "We are going to supply all the possible information that we can". "People evidently are misunderstanding a lot about calorimetry. A lot of people are making calorimetric measurements that may not be suitable for these experiments". Pons has repeatedly cautioned that some labs have been in too big a hurry to get confirmations. "We have had abundant criticism ..... primarily from physicists who have been having a good laugh in recent weeks at the expence of the researchers, the U and the State of Utah" "Fantastic, no problem. Criticism does not bother me. What bothers me is that these people are not doing the science. They are philosophizing, using a physical theory, parts of which may be totally wrong. Prey, where is their data? We are presenting ours. Why don't they publish their experimental procedures so that we can see what they are doing to disprove our work. It is very easy to pontificate and to say someone's data is wrong; proving it is another matter". Pons said he and Fleischmann haven't seen anyone disprove their data. "If they do, we will certainly admit it; if they don't, they will have to stand corrected".

Dr. Brophy said that it was "awfully difficult for me to believe that experimental electrochemists who have been working on their experiment for 5.5 years" could have their results dismissed by researchers who had worked on the problem for only five weeks.

It may be noted that most of these comments that differ from those of people outside Utah, come from Prof. Pons and Dr. Brophy but not from Martin Fleischmann.

3. COMPARISON WITH CHARACTERISTICS OF PATHOLOGICAL SCIENCE

The father of Pathological Science or the study of Wrong Results in Science, was the great physical chemist Irving Langmuir. I have been studying it since the splitting of the A2 that I was close to, some 20 years ago, and since 13 years have been giving occasional lectures on the subject after further work. Originally Irving Langmuir gave 6 characteristics by which one could recognise wrong results. However when I took a somewhat larger sample of cases and made a two dimensional array, I found that only 2 to 5 of the characteristics were fulfilled which was too weak statistically. Also I could identify 6 other characteristics. Then each case fulfilled about 6 to 8 of the 12 characteristics which was more satisfactory.

After hearing Martin Fleischmann's inspirational talk at CERN on 30 March, I found that his work only fulfilled two of the characteristics, namely
5. "Unusual or rare materials used"
12. "Unusual or surprising result".

Since then times have changed and more characteristics are satisfied;

7. "Understanding experiment may depend on not available details". Because of this I asked Steve Jones, if he had any special secret - he said "no" and laughed. Similarly on March 30, Martin Fleischmann gave the impression it was all simple and straight forward. To be sure, I asked him a few days later if there was any special way in which the rod had to be treated - he said no, it was a volume effect and therefore the surface treatment did not matter. However as has been shown above, Prof Pons suggests that the rod must be cast and not annealled, and Prof Huggins says the hydrogen must be driven out first.

6. "Great reluctance to change the experimental conditions". Although Fleischmann and Pons say they have been working on the subject for 5 1/2 years, it is amazing how few experiments they have tried whereas others have varied conditions and materials and salts and looked for dynamic rather than stable effects.

4. "Statistics small, special - no point in doing a statistical analysis". One would have expected hundreds of tests to have been made and statistics to have been presented, but the best we have is a recent statement that now 90% of the tests are successful - but the actual numbers are not given though it is hard to see why a patent lawyer would forbid it.

10. "Always a quick and ready answer to objections - though often hard to check answer". A perfect example is the 8mm diameter rod that was declared to be "dead", but the reasons given are hard to tie down or to test.

11. "The experiment develops in three phases;
Phase 1. the original results are presented and quickly confirmed by several other experimental groups.
Phase 2. gradually a few null results are presented and then there is a period of confusion where there are equal numbers of confirmations and null results.
Phase 3. All the results are null".

W4e have gone through Phase 1 and are in Phase 2. However this needs some explanation. If a group tries to confirm, it tends to do it quickly and in its rush, if it finds the effect, feels that it has acheived a good and correct result and therefore it is safe to publish (or in the modern way, to call a press conference). If however the group does a careful job making full checks and controls, and finds no effect, it is naturally reluctant to publish because;

a) it may have made a mistake since it disagrees with published results so that the penalty for error is greater

b) there may be some secret that only the original authors know.

So the group with a null result is slower to publish.

Once it becomes widely known that some people have found a null result, then all the other groups that have found null results and are holding back, now decide to publish. And the situation is reversed - the groups that found a positive confirmation now feel they should check further, and when they do proper checks, also find null results.

In this special case of Cold Fusion, there is a new factor. Communications are fairly fast nowadays, so that the big news story that Cold Fusion had been found, went round the world very quickly and everywhere groups tried to repeat the effect. In two regions of the world, North-Western Europe and USA Region 1, there is an excellent network of instantaneous communication so that people quickly realised that their neighbours also had trouble repeating the experiments. Since they had lots of excellent equipment already available they could do experiments quickly. So in these two regions 20 out of 21 groups (exception was Stanford), found and reported null results. In the other regions the teams had often less equipment available and their communications were less good so that out of perhaps a hundred groups, only those few with apparent confirmation, presented their results. The numbers of null results is increasing with time but at present it stands at (29-1) confirmations and 5 null results. The difference between the parts of the world is statistically significant.

There is as far as I can observe, a correlation between the media in the two parts of the world. In NW Europe and the greater NYT region, a substantial fraction of the media do not simply believe every press release, but check to see if outsiders consider it reasonable and if they agree. This healthy dose of scepticism is a great saving. So the result is that NW Europe and the USA Region 1 are in Phase 3 while other parts of the world are in Phases 1 and 2.

Thus 7 out of the 12 characteristics of a Wrong Result appear to be fulfilled and this is the normal proportion.

Characteristic number 9 - "Great confidence -You must believe" is almost fulfilled from what we have heard of the atmosphere in Utah (for which I thank correspondants there).

Generally in talking about Wrong Results in Science, I only treat cases of more than forty years ago to avoid personal feelings, but in view of the harm being done, this is a special case.

It is important to emphasise that there is no hint that anyone was deliberately cheating or falsifying data. In Pathological Science as in the cases Randi has dealt with, 95% of the people concerned are deceiving themselves - their desire and belief in the result is so strong that their judgement weakens and they ignore and do not seek for any negative evidence. The indications of paranoia shown in the quotations above, illustrate this. This week's quotation in CERN chosen by R. Carreras is from Nietzsche and says that "Convictions are more dangerous foes of truth than lies"

4. CONCLUSIONS

I hope there will be a wide discussion of all aspects of the subject by all parties at the Santa Fe Workshop and that the whole subject will quickly fade away after that though there will always be some discussion. In Pathological cases, the people who first produced the wrong result, generally never admit they made a mistake, except in a few cases like Paneth who was a great scientist. Thus Blondlot, the discoverer of N-Rays wrote a book afterwards, but everyone had already stopped discussing the subject.

After the turmoil of the last few weeks, it would be good to remember that we all make mistakes and to be humble and sympathetic.

Douglas R. O. Morrison

PS Have just received from Petros Rapidis the latest bulletin from Robert Park who says that the Santa Fe workshop will have a near capacity crowd of 1500. Although the preliminary programme gave top billing to Fleischmann and Pons, they seem to have decided to cancel their appearence (University lawyers apparently worried that Pons might reveal the secret formula) - hope this is not correct and that they do turn up to discuss in the normal scientific manner. He says the Santa Fe non-confrontation will be broadcast live by satellite.

Also Park reports that the Cold Fusion Panel of the Energy Research Advisory board will be co-chaired by physicist Norman Ramsey and nuclear chemist John Huizenga. The 20-man panel should give a preliminary report by Aug 31 and a final report by 15 Nov.

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May 17, 1989
(Via DHL Express)

Dr. S. Mercurio
Dipartimento di Ingetenria Nucleare
Viale Delle Science
Palermo
ITALY

Dear Dr. Mercurio:

Sorry my letter of 04/25/89 has not yet reached you through normal channels. Here it is, together with a copy of the Nature article on which I have corrected by hand errors that I corrected in proof, but which were ignored by the Editor.

I think it would be redundant to provide my original foils and text. If you agree, then I think you should reprint the Nature article with the introduction

> "Richard L. Garwin of IBM Research Division presented a paper 'Questions Provoked by the Experiments.' The substance of that paper has been published by him in the following Nature article of April 20, 1989."

If you absolutely prefer a paper in addition to the Nature article, please call once more, and I will let you have it in two days.

Very best regards, and sorry for the delay.

Sincerely yours,

Richard L. Garwin rsa

Richard L. Garwin
Forwarded in his absence

Encl:
+04/25/89 LTR RLG to S. Mercurio re Cold Nuclear Fusion. (042589..SM)
04/12/89 RLG foils at Erice meeting, "Questions provoked by experiments." (041289.RLG)

RLG:jtml:137%SM:051789..SM